HYPOTHESIS NON FINGO:

THE DEVELOPMENT OF ISAAC NEWTON'S LITERARY TECHNOLOGY

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ABSTRACT

This thesis examines a dispute between Isaac Newton and Robert Hooke during the 1670s over Newton's "New theory about light and colour." The controversy offers a fascinating window into the development of Newton's literary methodology for the presentation of his experimental facts. As such, I trace a transition from the genteel natural philosophy of Robert Boyle to the origins of modern scientific objectivity. While early modern science is often seen as the pursuit of obscure knowledge by natural philosophers in the privacy of their laboratories, such a view fails to recognize adequately the role science played in the public sphere. It was not merely a matter of 'facts' uncovered by 'scientists' in their laboratories, but also one of public representation of these facts and the knowledge which had been deduced from them. Newton challenged the authority of the Royal Society by suggesting and developing alternative conceptions of experimental credibility, mathematical certainty and dissemination. I contend that these alternative conceptions were a direct response by Newton to the controversies created by his optical theories. In order to avoid future disagreements, he tried to create a method of presenting his theories that would establish himself as authoritative. Thus, the dispute between Hooke and Newton played a key role that historians have hitherto failed to recognize in the shaping of the rhetorical methodology of science. By identifying and tracing the development of increasingly sophisticated literary technology, there is a great deal to be learned not just about seventeenth century natural philosophers but scientific writing as a whole. Through the close study of a specific dispute, such as between Hooke and Newton, we are able to learn more about the rhetorical methods used by scientists to establish their authority in regard to the knowledge they produce.

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Lastly and most importantly, I must thank Larry Stewart for introducing me to Isaac Newton and Robert Hooke.

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"It's hard to accept the idea that there cannot be an order in the universe because it would offend the free will of God and His omnipotence. So the freedom of God is our condemnation, or at least the condemnation of our pride."

– Umberto Eco*, The Name of the Rose*

“If, instead of this remark, my father had taken the pains to explain to me, that the principles of Agrippa had been entirely exploded, and that a modern system of science had been introduced, which possessed much greater powers than the ancient, because the powers of the latter were chimerical, while those of the former were real and practical; under such circumstances, I should certainly have thrown Agrippa aside, and, with my imagination warmed as it was, should probably have applied myself to the more rational theory of chemistry which would have resulted from modern discoveries. It is even possible that the train of my ideas would never have received the fatal impulse that led to my ruin. But the cursory glance my father had taken of my volume by no means assured me that he was acquainted with its contents; and I continued to read with the greatest avidity.”

– Mary Shelley, *Frankenstein*

Introduction

No one better exemplifies the image of the solitary scholar than Isaac Newton alone in his Cambridge rooms conducting trials with prisms. In his optical experiments Newton refracted rays of light in order to understand its composition. He performed them unassisted and without witnesses. While Newton took his inspiration from the likes of Rene Descartes, Robert Boyle and Robert Hooke, his experimental programmes, techniques and conclusions were all his own. Indeed, at times historians of Newton give the impression that he worked entirely in a vacuum. Though an exaggeration, when Newton began his prism trials in the 1660s such a characterization is not entirely unfair as he had very little direct contact with the world outside of Cambridge. This changed in 1672 when he submitted to the Royal Society the "New theory about light and colour," which summarized the results of his optical studies. The short article touched off a firestorm that entangled Newton for much of the rest of the decade until Newton finally gave up responding to his critics.

Today Isaac Newton (1642-1727) is one of the best known scientists in history. He is widely recognized for his genius and remembered primarily for his universal theory of gravitation. Furthermore, he was venerated during his own lifetime. He served as president of the Royal Society of London from 1703 until his death in 1727 and was knighted in 1704. By the eighteenth century his genius was essentially unchallenged in Britain and remained that way until the nineteenth century.[1] The "New theory about light

[1] On optics after Newton see Geoffrey Cantor, *Optics after Newton: Theories of Light in Britain and Ireland, 1704-1840* (Manchestor: Manchester University Press, 1983); Henry Steffens, *The Development of Newtonian Optics in England* (New York: Science History Publications, 1977). For a counter-narrative see

and colour," however, was his first publication. Prior to this his direct contact with natural philosophers had been limited to his predecessor as Lucasian Professor of Mathematics, Isaac Barrows, and to London book-seller and amateur mathematician John Collins. In 1672 Newton was only thirty years old and had held his position as professor of mathematics at Cambridge for just three years. His career up until that point had not necessarily cast him for greatness. He famously had entered Trinity College in 1661 as a "subsizar," forced to pay his way by performing menial labour for the fellows and wealthy students. This was despite the fact that Newton was heir to a manor and his mother's income exceeded £700 per year. According to Newton biographer Richard Westfall, she "begrudged" her son further education at all.[2]

While Westfall gave a great deal of attention to describing Newton's private studies while a Cambridge undergraduate, he also observed that Newton did not outwardly establish himself as anything other than an odd and intensely reclusive figure during this time.[3] After 1665, however, Newton became known to the first Lucasian Professor of Mathematics, Isaac Barrows, who regarded Newton as something of a protégé and did a great deal to advance Newton's early career.[4] Most significantly was Barrow's support for Newton to replace him as mathematics professor in 1669. Thus, at the age of twenty-seven Newton became comfortably ensconced in a position that allowed him to continue his solitary contemplation. Other than the period of controversy

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Casper Hakfoort, *Optics in the Age of Euler: Conceptions of the Nature of Light, 1700-1795* (Cambridge: Cambridge University Press, 1995).

[2] Richard Westfall, *Never At Rest: A Biography of Isaac Newton* (Cambridge: Cambridge University Press, 1980), 72-3.

[3] Westfall, *Never at Rest*, 74-5.

[4] Westfall, *Never at Rest*, 202-3.

between 1672 and 1678, he remained quietly isolated until after the *Principia* came out in 1687.

As Lucasian professor, Newton was required to give regular lectures of at least one hour per week during the term.[5] There is little evidence, however, that anyone attended them. Only three former Cambridge students claimed to have been instructed by Newton.[6] Though he was required to lecture every term, Newton only did so one term per year and erratically at best. He was just as likely to leave Cambridge in the midst of the term as not.[7] While obligated to deposit manuscript copies of ten lectures per year, he only did so four times through 1687 after which he ceased to lecture entirely, though he held his post fourteen more years in sinecure.[8] Regardless of his future lapses in lecturing, in 1670 he began his inaugural series of lectures and chose optics as his subject. This was an eccentric choice since optics was not regarded as mathematics.[9] Newton defended his decision by arguing for a definition of mathematics broad enough to be able to cover nearly any subject that interested him at any given time.[10]

A Cambridge professorship would seem to have been perfect for a man who apparently made only one acquaintance who could be described as a friend during the more than thirty years he spent at the university. Indeed, Newton spent most of his time as a student either alone in his rooms or on long, solitary walks around the campus.[11] In a

[5] Alan Shapiro, "Introduction," in *The Optical Papers of Isaac Newton, Vol. I, 1670-72*, edited by Alan Shapiro (Cambridge: Cambridge University Press, 1984), 16.
[6] Wesftall, *Never at Rest* 210.
[7] Westfall, *Never at Rest*, 211.
[8] Westfall, *Never at Rest,* 211.
[9] Alan Shapiro, "Experiment and Mathematics in Newton's Theory of Color," *Physics Today* 37 (1984), 36.
[10] Newton, *Optical Papers*, 439.
[11] Westfall, *Never at Rest*, 74-6.

letter to John Collins Newton primarily expressed a desire to maintain the "serene liberty" that his quiet professorship had afforded him.[12] Despite his disinclination toward publicity, however, he had begun to prepare his optical lectures for publication. When he submitted the "New theory about light and colour" for public scrutiny, he did so with the intention that it would herald the complete work that he anticipated would follow shortly. However, when he found himself faced with extensive criticism he quickly withdrew and did not publish again on optics until 1704.

The "New theory" was a response to previous theories regarding the composition of light and colour, especially the one advanced by Robert Hooke (1635-1703) in *Micrographia* (1665). While Hooke had proposed a wave theory of light, Newton argued that white light was a composite of all colours. As Newton explicitly rejected Hooke's theory, that Hooke took issue with it and responded quickly and aggressively is hardly surprising. It is important to recognize, however, that Newton's theory was highly controversial and that dispute was not limited to Robert Hooke. In addition to Hooke, Newton defended himself against significant critical responses from Christiaan Huygens, Ignace Pardies and a group at Liège, led by mathematics professor and English Jesuit Francis Line, his student John Gascoines and theology professor Anthony Lucas. I have chosen to focus primarily on the dispute between Newton and Hooke because it offers the most interesting and heretofore underexplored implications for historians of early modern science. The controversy was a clash of incompatible models of natural philosophy due to conflicting conceptions of certainty and the ultimate purpose of the new science. Newton

[12] Isaac Newton to John Collins, 25 May, 1672, in *The Correspondence of Isaac Newton, vol. I, 1661-1675*, edited by Henry Turnbull (Cambridge: Cambridge University Press, 1959), 161.

was consciously attempting to reframe natural philosophy, yet this aspect of the dispute is seldom recognized.[13]

There has been an unfortunate tendency for historians of Newton to detach him from the more general historiography of early modern natural philosophy. This likely largely has to do with the complexity and enormity of Newton's philosophical work. Thus, much of the scholarship that has been done on Newton's philosophy has been a product of intense zealots such as D.T. Whiteside, who devoted his entire career to editing for publication Newton's mathematical papers.[14] There has been a divide in which historians have generally either studied the work of Newton in and of itself or they have concentrated on the rise of "Newtonianism" in the eighteenth century.[15] Those who have bridged the gap have often largely focused on his theology rather than his more difficult mathematical and scientific writings.[16]

Newton exemplifies the "great man" narrative. Historians such as A. Rupert Hall fought to regard Newton as an exemplar of supreme intellect. To Hall the dispute with Hooke was not a battle between equals. Newton presented a theory of great genius and

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[13] Alan Shapiro has argued that Newton was attempting to assert his own model of natural philosophy, see Alan Shapiro, *Fits, Passions, and Paroxysms: Physics, Method, and Chemistry and Newton's Theories of Colored Bodies and Fits of Easy Reflection* (Cambridge: Cambridge University Press, 1993), 12-39.

[14] Isaac Newton, *The Mathematical Papers of Isaac Newton, vols. I-VIII,* edited by D.T. Whiteside (Cambridge: Cambridge University Press, 1967-82).

[15] For examples of the former see Richard Westfall, *Force in Newton's Physics: The Science of Dynamics in the Seventeenth Century* (London: Macdonald, 1972); Alan Shapiro, *Fits, Passions, and Paroxysms*. Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method* (Cambridge, MA: The MIT Press, 2009. For the latter see esp. Margaret Jacob, *The Newtonians and the English Revolution, 1689-1720* (Ithaca, N.Y.: Cornell University Press, 1976).

[16] For examples see Betty Jo Teeter Dobbs, *The Janus Faces of Genius: The Role of Alchemy in Newton's Thought* (Cambridge: Cambridge University Press, 1991); Stephen Snobelen, "'God of Gods, and Lord of Lords': The Theology of Isaac Newton's General Scholium to the Principia," *Osiris* 16, (2001): 169–208.

Hooke was unable to understand it.[17] Zev Bechler, however, ultimately redeemed Hooke's role in the controversy.[18] Instead of a misunderstanding brought upon by the confused and needlessly antagonistic Hooke, Bechler demonstrated that the dispute had greater significance than as an example of Newton's superiority over his contemporaries. If Hooke did not comprehend the "New theory" it was because Newton demanded that his audience accept a paradigm which was in conflict with Hooke's own and not because, as Hall implied, Hooke lacked the ability to follow Newton's brilliance. Westfall went further than Hall and suggested that even in his youth Newton far exceeded his peers at Cambridge. According to an anecdote given by Westfall, Newton "read the logic before he got to Cambridge and found that he knew more than the tutor."[19] In another, Westfall quotes Newton's niece Catherine Conduitt who alleged that even when playing checkers the young Newton was "unable to conceal his brilliance" and "if any gave him the first move" he was "sure to beat them."[20] The implication is clear; Newton was an individual of singular genius whose achievements were entirely a product of his brilliant mind.

Of course, when Newton arrived at Cambridge in 1661 he was ignorant of the kind of experimental natural philosophy that was being done by the fellows of the newly founded Royal Society. Attention to Newton's external influences has generally concentrated on his formative years during the early to mid-1660s while he was still a student at Cambridge. This has been aided by the presence of Newton's *Trinity Notebook*

[17] A. Rupert Hall, *Isaac Newton: Adventurer in Thought* (Oxford: Blackwell Press, 1992), 123-6; see also A. Rupert Hall and Marie Boas Hall, "Why Blame Oldenburg?" *Isis* 53 (1962): 482-91.
[18] Zev Bechler, "Newton's 1672 Optical Controversies: A Study in the Grammar of Scientific Dissent," in *The Interaction Between Science and Philosophy*, edited by Yehuda Elkana (Atlantic Highlands, NJ: Humanities Press, 1974).
[19] Westfall, *Never at Rest*, 83.
[20] Westfall, *Never at Rest*, 76.

which contains his notes on his early studies, thus giving historians a glimpse into both what he read and how he responded to it. However, this has left some historians with a predisposition to see Newton's conception of natural philosophy as having been largely complete by the time that he was wrested out of obscurity with the publication of the "New theory" in 1672. Such a view disregards any suggestion of dialectic or further intellectual growth. J.E. McGuire and Martin Tamny, for instance, suggest that Newton read what works of natural philosophy he regarded to be important and took from them what bits he deemed to be significant. Thus, they presume that by the time he turned to substantial independent experimental research, he was moving largely in his own direction and had little to no further external influence.[21]

While Newton certainly developed his own, unique experimental programme his early work was greatly inspired by the natural philosophers of the time. In the chronology recorded in 1726 by John Conduitt, Newton stated he had bought a prism in August 1665 to "try some experiments upon Descartes book of colours" [22] However, according to A. Rupert Hall, "it is certain" that it was instead *Touching Colours* that had influenced Newton to begin his own optical trials.[23] Indeed, Descartes had written no "book of colours." Furthermore, Newton's claim to have purchased his prism at the Sturbridge Fair was impossible as there had not been a fair outside of Cambridge in either 1665 or 1666 due to plague. Though Newton might have gotten the date and author wrong, the origins

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[21] J.E. McGuire and Martin Tamny, "Origin of Newton's Optical Thought and its Connection with Physiology." In *Certain Philosophical Questions: Newton's Trinity Notebook*, edited by J.E. McGuire and Martin Tamny (Cambridge: Cambridge University Press, 1983), 244.
[22] Richard Westfall, *Never at Rest: a Biography of Isaac Newton* (Cambridge: Cambridge University Press, 1980), 157; A. Rupert Hall, *Isaac Newton, Adventurer in Thought* (Oxford: Blackwell Press, 1992), 41.
[23] Hall, *Isaac Newton*, 41.

of the prism experiments do demonstrate Newton's critical engagement in a broader experimental culture.

Another example of Newton's work being seen as largely developing in isolation can be seen in Alan Gabbey's argument that the *Principia* was a treatise on mechanics. While Gabbey does recognize Boyle's influence, he does so only to argue that:

> Boyle's seemingly paradoxical coupling of natural philosophy and mechanical principles had been legitimated by [Isaac] Barrows and [John] Wallis, and was to find effective expression in Newton's *Principia*.[24]

Gabbey refers to Newton's predecessors only to argue that Newton developed his own model of natural philosophy. Furthermore, Gabbey's only reference to Newton's optics was in relation to the optical lectures that Newton gave at Cambridge between 1670 and 1672 and he did not mention the optical disputes at all.[25] Peter Dear, however, has made a substantial challenge to the prevailing story. Recognizing the difference between English and Continental philosophical traditions during the sixteenth and seventeenth centuries, Dear has argued that Newton derived much of his method from the mathematical approach that was *de rigueur* amongst his continental peers.[26] Rather than a method that was entirely a product of Newton's own mind, he was trying to import aspects of mathematical philosophy to England while maintaining the experimental rigour promoted by the Royal Society. I contend that Newton's philosophy did not emerge complete and *ab intra.* Instead, using the dispute between Newton and Robert Hooke as a case study, I

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[24] Alan Gabbey, "Newton's *Mathematical Principles of Natural Philosophy*: A Treatise on 'Mechanics'?" in *The Investigation of Difficult Things: Essays on Newton and the History of Exact Sciences in Honour of D.T. Whiteside,* edited by P.M. Harmon and Alan Shapiro (Cambridge: Cambridge University Press, 1992), 314.
[25] Gabbey, 312.
[26] Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995).

argue that the optical controversies of the 1670s played a fundamental role in shaping Newton's natural philosophy, experimental method and rhetorical strategies for the rest of his career.

The narratives described above generally presume that Newton's theory was developed in the 1660s then presented to the public in its complete and final form. Alan Shapiro, however, has shown that this was not the case. The optical lectures that Newton was preparing for publication between 1670 and 1672 were "outdated" by 1675.[27] In 1672 Newton envisioned a mathematical theory of light and colour not unlike the one he later proposed for gravitation; however, he ultimately found that he was unable to make it work.[28] By the time Newton published *Opticks* in 1704 it was a fundamentally different work than the manuscript suggested by his optical lectures. In Shapiro's reading of the story, however, the controversies served only to disturb Newton's "solitude and equanimity," which caused Newton to delay publishing his theories long enough to conclude they were flawed.[29] According to Shapiro, the dispute had little direct effect on the content of Newton's optics. Yet, presumably the mere act of being forced to defend his theories and explain himself in greater detail would have forced Newton to evaluate his ideas in a way that he would not have otherwise.

Much of Newton's optics remained "bereft of a history" because, as Alan Shapiro has observed, they were "'wrong'" thus making their study "unfruitful and unworthy of the great man."[30] Similarly, much of the history of the "New theory" has viewed the

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[27] Alan Shapiro, "Introduction," 21.
[28] Alan Shapiro, "Experiment and Mathematics."
[29] Alan Shapiro, "Introduction, 20-1.
[30] Alan Shapiro, *Fits, Passions, and Paroxysms,*" 3.

controversies, to quote Zev Bechler, "through the eyes of Newton, the victor of history."[31] There seems to have been an assumption that Newton's genius was ultimately too great for him not to 'win.' Rob Iliffe, however, has analyzed the process by which Newton's theory of gravitation became canon.[32] Rather than simply accepting that Newton's theory was adopted by his peers because it was 'true,' Iliffe has argued that it succeeded because of the promotional strategy that Newton adopted. The "New theory" has tended to be viewed from a rather pre-Thomas Kuhn conception of rationality. In his ground-breaking work, *The Structure of Scientific Revolutions* (1962), Kuhn shattered the last remains of the myth of teleological scientific progress. Prior to Kuhn, historians of science still tended to regard science as being a process of advancement from a less 'true,' less 'advanced' past to the present continuing *ad infinitum* into the future. Instead, Kuhn posited that science operates under paradigms that serve to outline the kind of questions that scientists ask and both define and limit the kind of answers that they are able to obtain. Ian Hacking has argued that 'traditional' (pre-1960s) philosophy of science regarded science as being outside history, by which he meant that it was seen as inherently rational in its function, method and approach.[33] Such a perspective cannot conceive to question why one theory would be successful as opposed to another because it assumes that there is a single 'right' theory that naturally wins out. What Iliffe has demonstrated with regard to the *Principia* was that Newton's theory of gravitation won

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31 Bechler, "Newton's 1672 Optical Controversies," 124.

32 Rob Iliffe, "'Is He Like Other Men?' The Meaning of the *Principia Mathematica*, and the Author as Idol," in *Culture and Society in the Stuart Restoration: Literature, Drama, History*, edited by Gerald Maclean (Cambridge: Cambridge University Press, 1995).

33 Ian Hacking, "The Rationality of Science After Kuhn," in *Scientific Inquiry: Readings in the Philosophy of Science*, edited by Robert Klee (Oxford: Oxford University Press, 1999), 216-20.

assent not simply because of Newton's brilliance, but because of the effective means by which it was presented and promoted to his audience. I argue that the difficulties Newton faced with the "New theory" played a fundamental role in shaping the approach he took with his future efforts that has not been adequately recognized.

Scientific controversies, as Simon Schaffer has recognized, inherently involve a "contest about authority."[34] As such, the dispute between Hooke and Newton necessarily went beyond matters of fact. Those who challenged Newton questioned his authority as a purveyor of knowledge. Schaffer, however, was primarily interested in how Newton's disputes demonstrate concerns of instrument and technique. He gave relatively scant attention to Hooke versus Newton since Hooke did not challenge Newton on either ground. Disputes play a key role in shaping rhetorical methodology as they force those involved to defend their approach as being *the* proper means obtaining useful knowledge.[35] Schaffer saw the matter as exemplifying "how experimental instruments play a central role" as the "resources which experimenters deploy in their struggles to achieve authority."[36] The quarrel between Newton and Hooke is of interest because it was not about material technology; instead, both combatants were explicitly aware that their credibility was at stake. Rather than a matter of proper technique or suitable instrument, Hooke and Newton presented two conflicting views as to what the goal of natural philosophy ought to be and what Newton's experimental facts meant.

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[34] Simon Schaffer, "Glass Works: Newton's Prisms and the Uses of Experiment," in *The Uses of Experiment: Studies in the Natural Sciences,* edited by David Gooding, Trevor Pinch and Simon Schaffer (Cambridge: Cambridge University Press, 1989), 67.
[35] See for example, Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985).
[36] Schaffer, "Glass Works," 67.

In the first chapter of this thesis my purpose is to provide a clear understanding of Isaac Newton's optical theory in the context of seventeenth century natural philosophy. This is in order to give a strong foundation so that the analytical and theoretical arguments of the chapters that follow are clear. As such, I have chosen to begin by providing a relatively straightforward narrative of the dispute between Newton and Robert Hooke. This approach is a recognition that I am responding to a highly specific historical event with an argument grounded in a particular historiographical tradition. In order to prevent confusion, it is most expedient to begin by dealing with the "New theory" and the dispute between Newton and Hooke in and of themselves. Early modern science cannot be seen simply as the pursuit of obscure knowledge by natural philosophers in the privacy of their laboratories. Such a view fails to recognize adequately the role of science in the public sphere. That an experiment has been conducted is meaningless if it is not disseminated. Thus, in the first chapter I describe the process by which Newton made his doctrine public and sought to win assent from his audience. Meanwhile, in the second and third chapters I expand my focus in order to consider the broader implications that can be drawn from a study of the "New theory."

In the second chapter, I concentrate on what Steven Shapin called "literary technology"—the rhetorical strategy by which one attains assent—in order to argue that Isaac Newton consciously fashioned the "New theory" in an effort to convince his audience of its credibility.[37] Shapin has made the counterintuitive suggestion that truth

[37] On literary technology see Steven Shapin, "Pump and Circumstance: Robert Boyle's Literary Technology," *Social Studies of Science* 14 (1984): 481-520; Simon Schaffer, "The Leviathan of Parsontown: Literary Technology and Scientific Representation," in *Inscribing Science: Scientific Texts*

itself can be regarded as a historical category. Rather than accepting the supposed conventional wisdom that "a social history of truth is not supposed to be possible," Shapin demonstrated that truth is in fact constructed.[38] Shapin has been a leading proponent of what Jan Golinski has called the "constructivist" outlook in the history of science. By constructivism he meant:

> That which regards scientific knowledge primarily as a human product, made with locally situated cultural and material resources, rather than as simply the revelation of a pre-given order of nature.[39]

Of course, post-social history, post-cultural history, post-postmodernity, to argue that truth is a social product is no longer revolutionary. What Shapin sought to accomplish was to explain the mechanism by which something became "fact." He took his lead from seventeenth century virtuoso, gentleman and natural philosopher par excellence Robert Boyle (1627-1791) and concluded that the answer lay in social status.[40] Boyle argued at great length that the question at the heart of credibility was not what should one trust but whom.[41] What Boyle determined and Shapin has thus argued was that seventeenth-century natural philosophers primarily ascribed credibility with genteel status. When

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and the Materiality of Communication, edited by Timothy Lenoir (Stanford: Stanford University Press, 1998).

[38] Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994), 3.

[39] Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science, 2nd ed.* (Chicago: University of Chicago Press, 2005), xvii.

[40] See Golinski, "Robert Boyle: Scepticism and Authority in Seventeenth-Century Chemical Discourse," in *The Figural and the Literal: Problems of Language in the History of Science and Philosophy,* edited by Andrew E. Benjamin, Geoffrey Cantor and John R.R. Christie (Manchester: Manchester University Press, 1987); John Harwood, "Scientific Writing and Writing Science: Boyle and Rhetorical Theory," in *Robert Boyle Reconsidered,* edited by Michael Hunter (Cambridge: Cambridge University Press, 1994). For a critique of Shapin's thesis see esp. Rose-Mary Sargent, *The Diffident Naturalist: Robert Boyle and the Philosophy of Experiment* (Chicago: University of Chicago Press, 1995). For an introduction to Sargent's argument see Sargent, "Learning From Experience: Boyle's Construction of an Experimental Philosophy," in *Robert Boyle Reconsidered.*

[41] See esp. Shapin, *Social History of Truth,* Ch. 3.

applied to Newton, however, the thesis rapidly becomes untenable. Newton was neither unambiguously genteel, nor did he equate social credibility with scientific. In *A Social History of Truth*, Shapin chose to approach seventeenth century natural philosophy broadly to ask "what is truth?" or at least what was truth to a certain community in the seventeenth century. Lorraine Daston and Peter Galison, on the other hand, have described the emergence of modern scientific objectivity during the nineteenth century.[42] In the development of Newton's method we can see the transition between the genteel credibility of Boyle to the impersonal authority of nature that ultimately arose. This thesis serves to bridge the gap between the historiography of Boyle and that of the rise of public science in the eighteenth century.

In the third chapter I use Robert Boyle, Robert Hooke and Isaac Newton as three separate, distinct models of natural philosophy in order to demonstrate an evolution from Boyle, who located authority in the person doing and reporting the experiments, to Newton, who sought to place the authority of his facts within the facts themselves. In 1672 Newton attempted to provide a piece of experimental philosophy that fit within the traditional mold as exemplified by Robert Boyle, albeit with some idiosyncratic changes. When Newton found the Boylean method unsuited for the degree of certainty he wished to elicit, he adapted his approach. It is in this revision that an examination of Newton's literary technology is embedded into the larger and more fundamental issues relating to objectivity and the location of authority that are the focus of the third chapter. Newton did not see his facts as credible because of his status, but because they were 'objective'

[42] Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007).

statements about reality. I conclude by describing the end of the controversies, which was provided by J.T. Desaguliers thirty years after they had begun. In doing so I demonstrate that the optical controversies were more than a conflict of laboratory discovery or personality. Instead, Newton played a key role that has hitherto gone unrecognized by historians in the transition from a niche natural philosophy, from genteel diversion to the so-called objective, impersonal, "a-historical" scientific model of today.

Chapter One
Newton v. Hooke, Round One: Optics, 1672

> "For the basic problem of philosophy seems to be to discover the forces of nature from the phenomena of motions and then to demonstrate the other phenomena from these forces."
> – Isaac Newton, *Principia*

Isaac Newton's "New theory about light and colour" was sent to Henry Oldenburg, secretary of the Royal Society, on February 6, 1672.[1] Newton's theory was the result of a series of experiments on optics conducted while he was still a student at Cambridge, approximately between 1665 and 1668, before he had become Lucasian Professor of Mathematics in 1669.[2] It was his first attempt at publishing and would be his only foray into article writing as his later works all took the form of the monograph.[3] Newton proposed a corpuscular conception of light in which it was composed of rays of unequal refraction. White light was thus made of a mixture of all colors and each separate color was formed by the angle of refraction. For Newton, this was demonstrated by observing the refraction of a ray of light through a prism. When he performed this experiment, the spectrum was projected onto the wall and he noticed that each color appeared in its own specific position in the spectrum. Because each colour had a measurable angle, he was convinced of the possibility of a mathematical theory of light

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[1] Isaac Newton to Henry Oldenburg, 6 February, 1671/2, in *The Correspondence of Isaac Newton, vol. I, 1661-1675,* edited by Henry Turnbull (Camridge: Cambridge University Press, 1959), 92-102. N.B. While the continent had modernized its calendar to begin the year on January 1, England was still on the old calendar, which did not begin the new year until March 25. For the sake of simplicity, I have modernized dates given in the body of the essay; however, in the footnotes I have cited Newton's letters with the convention of giving both years during the period of overlap in order to remain consistent with *Correspondence*.

[2] For the difficulties in precisely dating when Newton performed his experiments, see A. Rupert Hall, *Isaac Newton: Adventurer in Thought* (Oxford: Blackwell Press, 1992), 42-53.

[3] For a discussion of the articles versus the monograph in the career of Isaac Newton, see Charles Bazerman, *Shaping Written Knowledge: The Genre and Activity of the Research Article in Science* (Madison: University of Wisconsin Press, 1988), Ch. 4.

and color similar to the approach he would eventually propose for universal gravitation.[4] Once he became committed to his 'doctrine,' he set out to formulate its mathematical foundation. These mathematical demonstrations would form the bulk of his optical lectures; however, Newton considered the proof be shown by a single *experimentus crucis.* In this chapter, I seek to describe the process by which Newton attempted to make his optical theory public and win assent for it. For the most part, I will focus on the narrative details of his first publication and the dispute with fellow experimental philosopher Robert Hooke that ensued.

Isaac Newton was consciously trying to reframe natural philosophy into a model that possessed a more mathematical degree of certainty. While Peter Dear has considered the "New theory" to have been a failed attempt by Newton to meet the conventions of his time, I contend that, in fact, Newton actively sought to subvert said practices in order to promote his own model of mathematical natural philosophy.[5] Dear focused on the narrative form of early natural philosophy. His argument was that natural philosophers described their experiments in a detailed account of an event that actually occurred, which Newton failed to do. Dear's suggestion and its implications will be addressed in more detail in the second chapter. For the moment, the primary point I wish to make clear is that there were two major traditions at the time Newton was beginning his career: empirical experimental philosophy as represented by Robert Boyle and mathematical philosophy as practiced by continental natural philosophers. Rather than maintaining a strict division, Newton wished to expand on John Wilkins' efforts to merge the two

[4] Alan Shapiro, "Experiment and Mathematics in Newton's Theory of Color," *Physics Today* 37 (1984), 38.
[5] Peter Dear, "*Toitus in Verba*: Rhetoric and Authority in the Early Royal Society," *Isis* 76 (1985), 155.

methods into a “physico-mathematical” philosophy.[6] Though Newton ultimately did not obtain the degree of assent he desired in 1672, he learned from the experience and was more successful when he published his theory of gravitation in 1687. Thus, the controversies he experienced in the 1670s played a crucial role in the development of Newton’s literary technology and his methodology.

In 1671 Isaac Newton was still largely unknown. He had, however, begun his climb out of academic obscurity as his name had become familiar to a few persons of significance. Most important for Newton’s career up until that point had been the support of his predecessor as Lucasian Professor of Mathematics at Cambridge, Isaac Barrow, whose backing of Newton had been crucial to Newton obtaining the appointment at just twenty-seven years of age. Also key was Newton’s relationship with John Collins, whose wide circle of correspondence had begun to introduce Newton and his ideas to the world at large.[7] With Newton’s transmission of his early mathematical treatise *De analysi* to Collins in London, “Newton’s anonymity began to dissolve.”[8] Collins wrote diligently to Newton, posing mathematical questions and obtaining bits of Newton’s mathematical theories, most of which likely sailed well over Collins’ head.[9]

He may not have been able to understand much of Newton’s work, but Collins certainly was able to recognize Newton’s genius and the value he offered the world if he

[6] Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995), 2.
[7] Richard Westfall, *Never at Rest: a Biography of Isaac Newton* (Cambridge: Cambridge University Press, 1980), 202-3; Cf. Collins’ correspondence with James Gregory, *Correspondence, vol. I.*
[8] Westfall, 205-6.
[9] Westfall, 223.

would only divulge his work. Thus, Collins doggedly pressured Newton to publish, writing that:

> Your paines herein will be acceptable to some very eminent Grandees of the R Societie who must be made aquainted therewith, and forasmuch as Algebra may receive further Advancemt from your future endeavours.[10]

Collins understood that publishing would force Newton to exit the scholarly obscurity of Cambridge and establish his philosophical credentials. He had, however, initially misjudged Newton who did not particularly wish to be thrust into the public eye. Indeed, Newton saw "not what there is desirable in publick esteem, were I able to acquire and maintaine it. It would perhaps increase my acquaintance, ye thing wch I chiefly study to decline."[11] Though he remained ambivalent to publishing, Newton eventually submitted to pressure and provided the Royal Society with a brief description of his optical work on February 6, 1672. The fallout from this would have major ramifications for the rest of his career and occupied much of Newton's time for the next decade.

The so-called crucial experiment was the key component of Newton's "New theory" and was at the centre of the disputes that followed. An *experimentum crucis* was a term Robert Hooke had coined to describe an experiment that conclusively demonstrated the argument in question. While generally used to disprove a given theory, Newton positively employed the term to contend that the experiment he described showed his doctrine to be unimpeachable fact. It was significant that Hooke had created the term

[10] John Collins to Isaac Newton, 19 July, 1670, *Correspondence, vol. I,* 36.
[11] Newton to Collins, 18 February, 1669/70, *Correspondence, vol. I,* 27.

"*experimentum crucis.*"[12] Hooke's phrase superseded that of Francis Bacon, who had used "*Instantiae crucis*," which translates literally as "instances of the crossroads." It was only later that philosophy of science would make crucial experiments absolutely decisive.[13] The transition is significant to explain the vehemence of Hooke's response. As Ian Hacking has explained:

> The picture is that two theories are in competition and then one single test conclusively favours one theory at the expense of the other. Even if the victorious theory is not proved true, at least the rival is knocked out of action.[14]

By using the term "crucial experiment," Newton strongly implied Hooke's theory had been disproved regardless of his later denials that he had intended any such thing. To disprove a hypothesis with a single example is much more likely than to prove one. At best Newton's crucial experiment demonstrated that the "New theory" was highly probable. In order for it to have been a crucial experiment, Hooke argued, Newton would have had to demonstrate every possible counter to his theory were invalid, which was impossible to do with a single experiment. As far as Hooke was concerned Newton was misusing the term in order to make the "New theory" seem to be more strongly demonstrated than it actually was.

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[12] Schaffer, "Glass Works: : Newton's Prisms and the Uses of Experiment," in *The Uses of Experiment: Studies in the Natural Sciences*, edited by David Gooding, Trevor Pinch and Simon Schaffer (Cambridge: Cambridge University Press, 1989), 74.

[13] Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983), 249.

[14] Hacking, *Representing*, 249. For a historicized challenge to the idea that only one theory can, or should, effectively exist at one time see Bruno Latour, *Pasteurization of France*, translated by Alan Sheridan and John Law (Cambridge, MA: Harvard University Press, 1988); Cf. Lee Smolin, *The Trouble With Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next* (Boston: Houghton Mifflin Harcourt, 2006).

Newton's experiment itself was simple enough (see Figure 1-1). Newton took two boards and placed them twelve feet apart from each other. He placed a prism at the window so that a ray of light was refracted and the spectrum was cast on the first board.

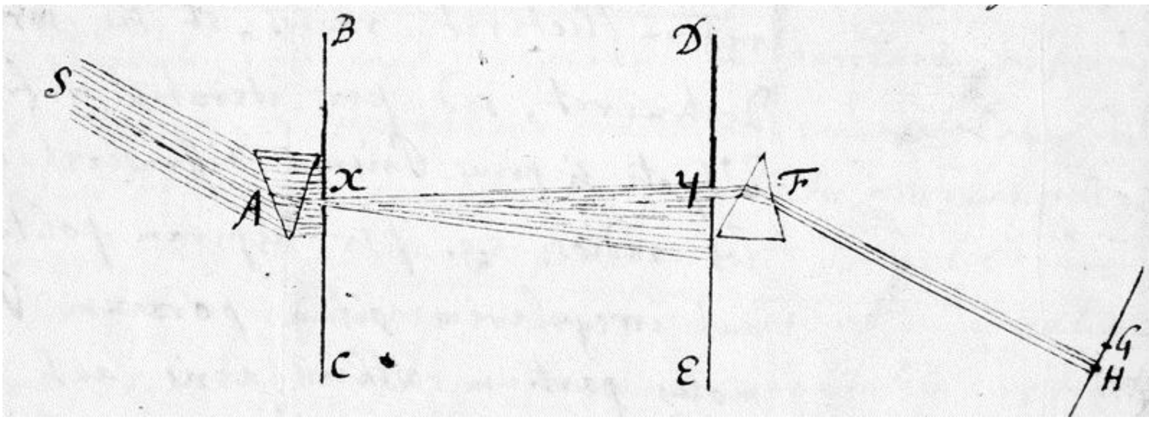

Figure 1-1
Isaac Newton's diagram of the 'crucial experiment'[15]

A second hole was drilled into this board so that a beam of light would be able to pass through it and fall on the second board, which had another small hole in it. Behind the second board Newton placed another prism. Thus, the ray of light passed through both boards and two prisms before finally reaching the wall. He then slowly turned the first prism around in his hand, thereby changing the angles, and observed the results. What he saw was that the second refraction was "considerably greater" than the first. He concluded that:

> The true cause of the length of that Image was detected to be no other, then that *Light* consists of *Rays differently refrangible*, which, without any respect to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards divers parts of the wall.[16]

[15] Newton to Oldenberg for Pardies, 10 June, 1672, *Correspondence, vol. I,* 166.
[16] Newton to Oldenburg, 6 February, 1671/2, *Correspondence, vol. I,* 95.

If all rays were equally refrangible, as was believed at the time, the image on the wall should have been nearly circular; instead, what Newton observed was that it was oblong. This led him to conclude he had found a "mathematical measure for color" in the degree of refrangibility.[17]

The construction of telescopes had been one of Newton's primary concerns in the latter half of the 1660s. Newton framed the "New theory" around the issue of the refracting telescope, which he had spent several years attempting to improve. The prism experiments, and the conclusions he had drawn from them, had caused him to realize the refracting telescope was fatally flawed; therefore, it could not obtain degree of accuracy he required. Ultimately, he decided that to continue to construct telescopes in the ordinary way was futile because "their improvement is not to be expected from ye *well figuring* of Glasses as *Opticians* have imagined."[18] This was because the perfectibility of it was limited not by want of better lenses but:

> Because Light it self is a *Heterogenous mixture of differently refrangible Rays.* So that, were a glass so exactly figured, as to collect any one sort of rays into one point, it could not collect those also into the same point, which having the same Incidence upon the same Medium are apt to suffer a different refraction.[19]

However, Newton "despaired not of their improvement by other constructions."[20] Instead, he turned to reflection as a means of telescopic perfection. It was his 'invention' of the reflecting telescope that led to his election as a Fellow of the Royal Society in

[17] Alan Shapiro, "Experiment and Mathematics," 36.
[18] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 172.
[19] Newton to Oldenburg, 6 February, 1671/2, *Correspondence, vol I,* 95.
[20] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 172.

1672.[21] Zev Bechler has argued that the acclaim directed at Newton for his reflector was, in Newton's mind, mislaid. The reflector was not the crucial result; instead, it was "intended as a test piece for his notions of colours and light."[22] The reflecting telescope was the "direct practical outcome of the new theory" and served to demonstrate the necessity of a new theory as he showed previous understandings of optics to be inadequate, as well as the superiority of his theory.[23] With his new telescope, Newton had shown his theory to be imminently practical and useful.

What Newton had submitted to the Royal Society comprised only a small part of his larger optical theory. The origins of his optical thought can be traced from the presence of a notebook he began to keep in 1664 that has been dubbed the Trinity Notebook by historians. In it he recorded the books he was reading, which enables historians to track the somewhat eclectic process of his self-motivated philosophical education. Newton's first significant exposure to contemporary optics would seem to have come, unsurprisingly, from Rene Descartes.[24] As well, Newton's first investigations on refraction and notes on grinding lenses are found in an undated notebook that also includes his annotations on John Wallis's *Arithmetica infinitorum*. The investigations

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[21] While he was widely credited as the inventor of the reflector, Newton in fact took the idea directly from a number of earlier sources and was 'merely' the first to make one, see Zev Bechler, "'A Less Agreeable Matter': The Disagreeable Case of Newton and Achromatic Refraction," *The British Journal for the History of Science* 8 (1975): 101.
[22] Bechler, "Less Agreeable Matter," 104.
[23] Bechler, "Less Agreeable Matter," 102.
[24] J.E. McGuire and Martin Tamny, "Origin of Newton's Optical Thought and its Connection with Physiology," in *Certain Philosophical Questions: Newton's Trinity Notebook*, edited by J.E. McGuire and Martin Tamny (Cambridge: Cambridge University Press, 1983), 244; For a description of Descartes' optical experiments with prisms see A. Rupert Hall, *All Was Light: An Introduction to Newton's* Opticks (Oxford: Clarendon Press, 1993), 9-11.

themselves were inspired by Descartes's *Dioptrics*.[25] Newton may have been thinking about optics prior to his reading of Descartes; however, it was from Descartes that he derived much of his early notions about light and colour.[26] Sometime between late 1664 and early 1665 Newton also read Robert Boyle's *Experiments and Considerations touching Colours* (1663). According to A. Rupert Hall, it was *Touching Colours* that inspired Newton's prism experiments though in later years Newton suggested that the Descartes as the source.[27] While Boyle's work introduced to Newton experimental natural philosophy, Boyle did not proffer an optical theory. Shortly after reading *Touching Colours* Newton encountered Robert Hooke's *Micrographia* (1665), which did propose an explanation of light. Newton studied *Micrographia* quite closely, as demonstrated by the fourteen printed pages of notes he took on it.[28]

Hooke introduced Newton to a number of new phenomena. For Newton, the most significant of these phenomena was the way in which new colours were formed when light shone through or were reflected from a transparent plate. Newton saw this as being the results of differing angles of refraction. Despite Hooke's priority, the phenomenon would come to be known as "Newton's rings."[29] However, *Micrographia* was significant to Newton's development for reasons beyond introducing him to a number of important optical phenomena. It motivated him to begin to develop more broadly his own

[25] McGuire and Tamny, 264.
[26] McGuire and Tamny, 265.
[27] Hall, *Isaac Newton,* 41.
[28] Hall, *Isaac Newton,* 50.
[29] Hall, *Isaac Newton,* 50. In the "New theory" Newton briefly acknowledged his debt to Hooke, see Newton to Oldenburg, 6 February, 1671/2, *Correspondence, vol. I,* 99.

experimental programme and to deviate from established thought. Hooke had proposed what was essentially an early wave theory of light, arguing that light was:

> A vibration or stream of pulses in an omnipresent aether, penetrating all bodies. White light, directly radiating from its hot source, consisted of a perfectly regular, uniform sequence of pulses (like a pure musical tone). When the sequence was disturbed, in a variety of ways described by Hooke, the non-uniform pulses created the appearance of colours.[30]

In his notes, Newton preferred Descartes' ideas to Hooke's hypothesis. This is somewhat ironic considering the later antagonism between Newtonians and Cartesians.

Robert Hooke responded quickly to Newton's paper. Hooke was all but obligated to do so because Newton's theory sought to refute his own. As Steven Shapin has demonstrated, a direct repudiation of another's matters of fact was in violation of the genteel codes of conduct under which the Royal Society sought to operate.[31] However, Hooke's motivation for entering into controversy with Newton went well beyond simply a desire to defend his own theory. Hooke's criticism of Newton focused primarily on a differing of opinion on the key concepts of hypotheses, conjecture, theory and certainty. He subscribed to a probabilistic notion of certainty based on the work of Robert Boyle, for whom he had served as an assistant before becoming curator of experiments for the Royal Society in 1662.[32] Barbara Shapiro has suggested that out of the English Common

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[30] Hall, *Isaac Newton,* 52.

[31] Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994). See esp. discussion of the C1/C2 comet controversy, 266-91.

[32] On Robert Boyle's conception of certainty, see Rose-Mary Sargent, *A Diffident Naturalist: Robert Boyle and the Philosophy of Experiment* (Chicago: University of Chicago Press, 1995); on certainty and probabilism in England more generally, see Henry van Leeuwen, *The Problem of Certainty in English Thought, 1630-1690* (The Hague: Martinus Nijhoff, 1963); Ian Hacking, *The Emergence of Probability: A Philosophical Study of Early Ideas about Probability, Induction and Statistical Inference* (Cambridge: Cambridge University Press, 1975); Barbara Shapiro, *Probability and Certainty in Seventeenth-Century England: A Study of the Relationships Between Natural Science, Religion History, Law and Literature* (Princeton: Princeton University Press, 1983).

Law came a different means of assessing certainty than continental countries that followed Romano-Canon Law because the English system did not have mechanical rules for such assessments.[33] The English common law differed from Romano-canon law in a number of ways, the most significant being how it dealt with proof. Roman law had a highly mechanical system in which witnesses were interviewed privately and then their testimony was submitted in written form to the judge who added up the evidence according to a set scale. If enough points were achieved, a matter was deemed to have been proven. Common law, on the other hand, had public trials and a jury of peers. Thus, English law emphasized testimony and it was the jurors' responsibility to assess the credibility of witnesses as well as the force and effectiveness of their testament.[34]

The English legal system allowed for differing levels of certainty. To be worthy of obtaining assent, a 'fact' did not have to be absolutely certain. A similar understanding as to what was sufficient proof for the judging of experimental facts was adopted by experimental natural philosophers in the seventeenth century. Shapiro has argued that, in its legal usage, a fact was only considered to be suitable to be believed after satisfactory evidence had been presented. A matter of fact was an issue placed before a jury involving whether or not an act had been performed by a certain person. A 'fact' was not an established truth, but an alleged act the occurrence of which was in contention.[35] By giving assent to a matter of fact, one implicitly accepted the authority and credibility of the claimant.

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[33] Barbara Shapiro, *A Culture of Fact: England, 1550-1720* (Ithaca: Cornell University Press, 2000).
[34] Rose-Mary Sargent, *Diffident Naturalist*, 45.
[35] Barbara Shapiro, *Culture of Fact*, 11.

Hooke was only one of a number of natural philosophers to dispute Newton's theory. Amongst others, Newton received challenges from Christiaan Huygens, Ignace Pardies, Giovanni Rizzetti, and a group of English Jesuits at Liège, led by mathematics professor Francis Line, his student John Gascoines and theology professor Anthony Lucas. Newton attempted to respond to their various criticisms in order to convince them of the veracity of his theory. The disputes with Hooke, Huygens and Pardies all reflected what Bechler has described as Newton's "dogmatic" approach.[36] Meanwhile, the Liège and Rizzetti disagreements had primarily to do with experimental method and his opponents' inability to replicate his crucial experiment.[37] The debate between Hooke and Newton contains both the fullest expression of Newton's thought and the most detailed Boylean criticism of it.

While Hooke had a more personal motivation than Newton's continental combatants, his criticisms were not simply a defence of his own theory. Hooke himself claimed that, "I doe assure him I am soe far from being concernd for any notion of Hypothesis of myne that I shall heartily thank any one that shews me better, or the defects of those."[38] His purpose, Hooke asserted, "'twas not to establish this or that hypothesis but to shew Mr. N. Corpuscular hypothesis of light and colours Not absolutely necessary."[39] An "Excellent Discourse of Mr Newton about colours and Refractions"

[36] Bechler, "Newton's 1672 Optical Controversies: A Study in the Grammar of Scientific Dissent," in *The Interaction Between Science and Philosophy,* edited by Yehuda Elkana (Atlantic Highlands, NJ: Humanities Press, 1974), 132.
[37] See Schaffer, "Glass Works."
[38] Robert Hooke to Lord Brouncker, c. June, 1672, *Correspondence, vol. I,* 198.
[39] Hooke to Lord Brouncker, c. June, 1672, *Correspondence, vol. I,* 200.

though the “New theory” might have been, Hooke was not convinced.[40] He went on to reiterate his own theory:

> For all the expts & obss: I have hitherto made, nay and even those very expts which he alledged, doe seem to me to prove that light is nothing but a pulse or motion propagated through a homogeneous, uniform and transparent medium: And that colour is nothing but the disturbance of yt light by the communication of that pulse to other transparent mediums, that is by the refraction thereof: that whiteness and blackness are nothing but the plenty or scarcity of the undisturbed Rayes of light; and that two colours (then which there are noe more uncompounded in Nature) are nothing but the effects of a compounded pulse or undisturbed propagation of motion caused by Refraction.[41]

As far as Hooke was concerned, Newton’s evidence was as much in favour of his own theory as it was of Newton’s and was not a refutation at all. Hooke contended that the “same phænomenon will be salved by my hypothesis as well as by his without any manner of difficulty or straining.”[42] When Newton eventually responded to Hooke, he did not dispute this point. Indeed, he saw the “affinity” between his theory and Hooke’s to be a strength and not a weakness.[43] Furthermore, Hooke added that he “will undertake to shew an other hypothesis differing from both his & myne, yt shall do the same thing.”[44] Hooke accepted that light was refracted as Newton described, what he “understood not” was “the necessity” of Newton’s explanation.[45] In Hooke’s mind, Newton had gone too far in the assertion of his theory. Rather than proposing a hypothesis, as Hooke had done, Newton presented his theory as indisputable fact.

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[40] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 110.
[41] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 110.
[42] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 111.
[43] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I*, 174.
[44] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 111.
[45] Hooke to Oldenburg, 15, February, 1671/2, *Correspondence vol. I,* 111.

The crux of the dispute between Newton and Hooke is found in a passage that Henry Oldenburg had omitted from the published version in an effort to head off controversy. Hooke, however, had received an advance 'review' copy in which the offending passage remained.[46] In it, Newton made some of his most dogmatic claims regarding a mathematical natural philosophy:

> In the last place I should take notice of a casuall expression wch intimates a greater certainty in these things then I ever promised, viz: The certainty of *Mathematical Demonstrations.* I said indeed that the *Science of Colours was Mathematical & as certain as any other part of Optiques*; but who knows not that Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematical Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by wch I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physicall*: Whence the Propositions themselves can be esteemed no more then *Physicall Principles* of a Science. And if those Principles be such that on them a Mathematician may determin all the Phænomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in wch severall colours are originally inherent; I suppose the *Science of Colours* will be granted *Mathematical* & as certain as any part of *Optiques*. And that this may be done I have good reason to believe, because ever since I became first acquainted with these Principles, I have with constant successe in the events made use of them for this purpose.[47]

In Hooke's response he stated that he could "see noe reason why Mr. N. should make soe confident conclusion.[48] Even more inappropriately, in another passage censured by Oldenburg, Newton had earlier stated in "New theory:"

> What I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjecturing by barely inferring 'tis thus because not otherwise or because it satisfies all phænomena (the Philosophers universall

[46] Alan Shapiro, "Experiment and Mathematics," n. 2; Cf. *Correspondence, vol. I,* 190, n. 18.
[47] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 187-8.
[48] Hooke to Lord Brouncker, c. June, 1672, *Correspondence, vol. I,* 202.

> Topick) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt.[49]

Hooke sought to demonstrate that Newton's experiment was "not an *Experimentus crucis.*"[50] As Hooke explained:

> [Newton] doth not bring any argument to prove that all colours were actually in every ray of light before it has sufferd a refraction, nor does his *experimentum Crucis* as he calls it prove that those proprietys of colourd rayes, which we find they have after their first Refraction, were Not generated by the said Refraction…All he doth prove by his *Experimentum Crucis* is that the colourd Radiations doe incline to ye Ray of light wth Divers angles, and that they doe persevere to be afterwards by succeeding mediums diversly refracted one from an other in the same proportion as as at first, all wch may be, and yet noe colourd ray in the light before refraction; noe more then there is sound in the air of the bellows before it past through the pipes of ye organ.[51]

Refuting the crucial experiment was not tantamount to repudiating Newton's theory; instead, it was a rejection of the overly dogmatic language of Newton's literary technology.

As Simon Schaffer has shown, the continental disputes primarily had to do with experimental technique and proper instrumentation.[52] Hooke, on the other hand, attacked Newton's interpretation. Indeed, Hooke did "wholy agree wth him as to the truth of those he hath alledged."[53] Anthony Lucas challenged Newton's theory because he was unable to replicate Newton's experiment, a problem that Hooke did not share as he had successfully repeated it. However, Hooke concluded:

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[49] Newton to Oldenburg, 6 February, 1671/2, *Correspondence, vol. I,* 96-7.
[50] Hooke, "Memorandum by Hooke," 19 June, 1672, *Correspondence, vol. I,* 195.
[51] Hooke to Lord Brounker, c. June, 1627, *Correspondence, vol. I,* 202-3.
[52] Schaffer, "Glass Works," 87-91.
[53] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I, 110.*

> As having by many hundreds of tryalls found them soe, yet as to his Hypothesis of salving the phænomena of Colours thereby I confesse I cannot see any undeniable argument to convince me of the certainty thereof.[54]

Hooke's problem with Newton was not merely that Newton had challenged his own theory, but that he had done so with a degree of certainty that was anathema to the proper conduct of natural philosophy, stating that:

> How certaine soever I think myself of my hypothesis, wch I did not take up without first trying some hundreds of expts; yet I should be very glad to meet with one Experimentum crucis from Mr Newton that should divorce me from it.[55]

Hooke denied the decisive role of the crucial experiment and argued that Newton's explanation was no more demonstrated than his own:

> I agree with the observations of the 9th, 10 and 11th though not wth his theory; as finding it not absolutely necessary, they being as easily and naturally explained & salved by my hypothesis…I doe not therefore see any absolute necessity to believe his theory demonstrated, since I can assure Mr Newton I cannot only salve all the Phænomena of Light and colours by the Hypothesis I have formerly printed and now explicate yt by, but by two or three others very differing from it.[56]

Proper method was to perform a large number of different experiments and to report them in full. Newton, on the other hand, only hinted at the existence of a greater body of experiments while not bothering to include them in his report because "to continue the historicall narration of these experiments would make a discourse too tedious & confused."[57] Indeed, in Newton's response to Hooke, he stated that he could have "acquainted him with my successes in the tryalls I have made of that kind" if Hooke had

[54] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 110.
[55] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 110-1.
[56] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I, 113.*
[57] Newton to Oldenburg, 6 February, 1671/2, *Correspondence, vol. I,* 97.

"obliged me by private letter"[58] Newton had failed to meet the conventions laid out by Boyle, who stressed that theories needed to be based on a large history of experiments designed specially to test them.[59]

Newton repeatedly emphasized in the Liège dispute that the only relevant experiment was the 'crucial' one and their efforts were in vain because they failed to follow his method. If an experimenter could not make the experiments work "it must be due to their wilful incompetence rather than to subtle differences in technique."[60] Hooke, on the other hand, argued that he could provide a number of experiments that suggested an alternative explanation to light and colour. If they seemed "at first sight much to confirm Mr Newtons Theory,"[61] ultimately:

> these experiments were not cogent to prove, that light consists of different substances or divers powders, as it were; but that these phænomena might be explained by the motion of bodies propagated.[62]

Hooke did not presume to refute Newton's theory; instead, he attempted to demonstrate that his own remained equally viable.

Hooke had offended another principle of Newton's when he stated that "nor would I understood to be said all this against his theory as it is an hypothesis."[63] This upset Newton because he understood 'hypothesis' much more strictly than did Hooke. For Hooke, hypothesis and theory were essentially interchangeable words, both of which

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[58] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 172.
[59] Rose-Mary Sargent, "Learning from Experience: Boyle's Construction of an Experimental Philosophy," in *Robert Boyle Reconsidered,* edited by Michael Hunter (Cambridge: Cambridge University Press, 1994), 64.
[60] Schaffer, "Glass Works," 88.
[61] Memorandum by Hooke, 19 June, 1672, *Correspondence, vol. I,* 195.
[62] Thomas Birch, *The History of the Royal Society of London, vol. III* (London, 1756-7), 50.
[63] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 113.

were only able to be proven to be probably, but not absolutely, true. After all, according to Hooke, "noething conduces soe much to the advancement of Philosophy as the examining of hypotheses by experiments & the inquiry into Experiments by hypotheses."[64] Newton, however, viewed theories as being the demonstrably true results of experiment while hypotheses were only conjecture. Much later, in the General Scholium of the 1713 edition of the *Principia,* Newton would famously write "hypothesis non fingo"—"I do not feign hypotheses"—and he would go on to explain:

> For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction.[65]

As Alan Shapiro has observed, to Newton "there were rigidly established, 'true' theories—like his theory of color—and there were hypotheses and the two must not be confused."[66] Thus, Newton quickly sought to clarify the distinction in his response:

> 'Tis true that from my Theory I argue the corporeity of light, but I doe it without any absolute positivenesse, as the word *perhaps* intimates, & make it at most but a very plausible consequence of the Doctrine, & not a fundamentall supposition.[67]

Newton did not reject the value of hypotheses in natural philosophy; indeed, he made frequent use of them. For instance, in July, 1672 Newton wrote to Henry Oldenburg suggesting eight "Quaeries" for his opponents to investigate further.[68] Newton believed that such experiments would demonstrate that his theory was "evinced" to him "*not by*

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[64] Hooke to Lord Brounker, c. June, 1672, *Correspondence, vol. I,* 202.
[65] Newton, *The Principia: Mathematical Principles of Natural Philosophy,* translated by I. Bernard Cohen, Anne Whitman and Julia Budenz (Berkely: University of California Press, 1999), 943.
[66] Alan Shapiro, *Fits, Passions, and Paroxysms: Physics, Method, and Chemistry and Newton's Theories of Colored Bodies and Fits of Easy Reflection* (Cambridge: Cambridge University Press, 1993), 22.
[67] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 173.
[68] Newton to Oldenburg, 6 July, 1672, *Correspondence, vol. I,* 209-10.

inferring tis thus because not otherwise" but "*by deriving it from Experiments concluding positively & directly.*"[69] It was crucial to Newton that hypotheses be clearly indicated as such and set aside from the experimentally proven facts.[70] He sought to emphasize this point, writing "And I wonder how Mr Hook could imagin that when I had asserted the Theory with the greatest rigor, I should be so forgetfull as afterwards to assert ye fundamentall supposition it selfe with no more than a *perhaps.*"[71]

Robert Boyle regarded the role of the natural philosopher as performing the task of 'under-builder' in which a foundation of an experimental history of a large number of different experiments would be built.[72] Isaac Newton was unwilling to accept such a vague definition of certainty and preferred a position not dissimilar to that expressed by Thomas Hobbes. Hobbes rejected Boyle's method in which a fact was nothing "but sense and memory," that "could not generate the kind of certainty appropriate to philosophical inquiries."[73] At the same time, however, Newton was dedicated to an experimental natural philosophy, something Hobbes had been fiercely against. Meanwhile, Boyle opposed mathematical natural philosophy because he thought it would restrict the size of the philosophic community. As well, he saw the mathematician's quest for certainty as lacking "philosophic point, purpose and decorum."[74] Mathematics was inappropriate, he

[69] Newton to Oldenburg, 6 July, 1672, *Correspondence, vol. I,* 209.
[70] The most clear example of this being the Queries of *Opticks*, which occur at the end, obviously separated from the rest of the book and clearly marked as hypotheses waiting to be 'proven.'
[71] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I, 173-4.*
[72] Sargent, "Learning from Experience," 58; Shapin & Schaffer, 66. The example of this Boyle liked to use was that of William Harvey's discovery of the circulation of blood and the way it was eventually proven over Rene Descartes' rival theory, see Sargent, *Diffident Naturalist,* 80-4.
[73] Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985), 101; 146-7.
[74] Shapin, *Social History of Truth,* 317.

believed, because it encouraged overly dogmatic assertions and philosophic systems building. Steven Shapin has observed that Boyle never attempted:

> To specify the shapes of different corpuscles, identify their geometric arrangements, quantify their states of motion…if one wished to claim that doing so is definitive of mechanical explanation, then one would have to conclude that Boyle never gave a mechanical explanation in his life.[75]

This was in clear contrast to the approach taken by Newton, who did just such measuring and quantifying.

Robert Hooke's initial response to Newton's theory was sent to Oldenburg on February 15, 1672—just nine days after Newton had submitted his theory to the Royal Society. Newton, however, did not deign to respond to Hooke's criticisms until June 11, and did so only after prodding from Oldenburg. The delay in Newton's response was a clear violation of the polite conventions of natural philosophy. Oldenburg wrote to Newton in April gently suggesting that "It would be well if your answer to Mr Hooks objections could be ready."[76] Even then it was another two months before Newton had an answer prepared. It is possible that Newton had found Hooke's comments particularly distasteful and had hoped to avoid responding at all. He had, after all, replied quickly to the other criticisms he had received.[77] A. Rupert Hall and Marie Boas Hall regarded Newton's response to Hooke as having been a masterful refutation of Hooke's objections. As far as Hall and Hall were concerned, Hooke's theory was "destroyed."[78] Seeing as Newton failed to end the dispute, such an interpretation credits him too much. The argument could be made that Hooke had too much invested in the dispute to give in.

[75] Shapin, *Social History of Truth,* 334
[76] Oldenburg to Newton, 9 April, 1672, *Correspondence, I,* 135.
[77] A. Rupert Hall and Marie Boas Hall, "Why Blame Oldenburg?" *Isis* 53 (1962), 484.
[78] Hall and Hall, 490.

Moreover, Hooke was hardly one for admitting defeat.[79] Certainly, Hall and Hall have pointed to the "beautiful tautology" contained in Hooke's claim to "heartily thank anyone who shews me better" as Hooke always saw his own hypotheses to be "better and he defended them bitterly."[80] Yet, Newton remained in controversy with a number of his continental challengers as well. The dispute was not definitively brought to a close until 1714, and it was Newton's disciple, J.T. Desaguliers, rather than Newton, who provided the demonstrations that did so.[81]

Newton opened his response to Hooke with the confident assertion that he had found nothing in Hooke's *Considerations* that he "conceived might not wthout difficulty be answered."[82] He began his attack by defending his decision not to include the entirety of his optical trials and to take Hooke to task for not requesting descriptions of the full trials via private letter.[83] He then moved on to the "*Theoretique part*" in which he vigorously asserted his views regarding hypotheses and certainty. While Newton's Trinity Notebook was fairly critical of Hooke's theory, in his June 11 letter, Newton sought to be somewhat more charitable and emphasized that his theory and that of Hooke were not entirely incompatible. Newton "understood not why Mr Hook should endeavour so much to oppose it. For certainly it hath a much greater affinity with his own

[79] For an example of Hooke's dogged persistence in the face of dispute, see Rob Iliffe's account of Hooke's dispute with Christiaan Huygens regarding the priority for the balance spring watch, Rob Iliffe, "'In the Warehouse': Privacy, Property and Priority in the Early Royal Society," *History of Science* 30 (1992): 29-68.
[80] Robert Hooke to Lord Brouncker, c. June, 1672, *Correspondence, vol. I,* 198; Hall and Hall, 486.
[81] Henry Guerlac, *Newton on the Continent* (Ithaca: Cornell University Press, 1981), 121-2.
[82] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 124.
[83] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 125.

Hypothesis then he seems to be aware of."[84] The language used by Newton would seem to indicate that he was concerned with more than a simple defence of his theory in itself; instead, he saw Hooke as challenging his experimental model itself. This becomes especially clear when contrasted with Simon Schaffer's discussion of the way in which Newton dealt with the continental disputes. In Newton's controversy with the Italian natural philosopher Giovanni Rizzetti, what was on the line was the veracity of the facts themselves as Rizzetti was unable to replicate Newton's experiment.[85] The facts were never a matter of contention for Hooke; instead, it was the interpretation that was in question.

Much of the reason why Hooke and Newton were unable to resolve their dispute was because they essentially spoke entirely different languages. This can be seen especially clearly in Newton's reply to Hooke. Newton expressed amazement that Hooke was "so much concerned for an *Hypothesis*, from whome in particular I most expected an unconcerned & indifferent examination of what I propounded."[86] According to Zev Bechler, "what [Newton] could not see was that no one disputed the truth of the facts or the *adequacy* of the hypotheses."[87] Bechler was primarily concerned with rescuing Hooke and the others who disputed with Newton from the condescension of Newtonian scholars who tended "to view the situation through the eyes of Newton, the victor of history."[88] Thus, Newton's opponents such as Hooke were given an "absurd" lack of respect by historians who wrote of these opponents "failure to grasp" the "depth" of Newton's

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[84] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I, 125.*
[85] Schaffer, "Glass Works," 96-98.
[86] Newton to Oldenburg, 11 June, 1672, *Correspondence, vol. I,* 171.
[87] Bechler, "Newton's 1672 Optical Controversies," 124.
[88] Bechler, "Newton's 1672 Optical Controversies," 124.

theory. For example, Hall and Hall dismissed Hooke as trying to "refute what he did not understand."[89] Bechler rightly corrected such a glib view toward Hooke and others, like Huygens and Pardies, who had taken issue with Newton's claims. All were highly accomplished and their responses clearly indicate they understood Newton's theory perfectly well. However, Bechler went too far in the other direction in his assertion of Newton's supposed "failure to grasp the exact import of the dispute."[90] It would have been equally unlikely that Newton would not have been able to understand the position of his opponents. Newton was "well acquainted with classical rhetorical techniques."[91] Certainly he would not have been ignorant of the anti-dogmatic stance of Robert Boyle upon whom Hooke, Huygens and Pardies based their criticisms.

As the Trinity Notebook has shown, Newton was quite well versed in the conventions of natural philosophy. By viewing the dispute through the lens of literary technology, I suggest that Newton was consciously trying to reframe natural philosophy into a model that possessed a more mathematical degree of certainty. That Isaac Newton was attempting to devise and promote a more mathematical conception of certainty and natural philosophy is hardly a novel argument; however, while Newton scholars have detailed Newton's mathematical philosophy, there has been a tendency not to take the argument outside of the narrow context of Isaac Newton himself.[92] As Newton was the

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[89] Hall and Hall, 490.
[90] Bechler, "Newton's 1672 Optical Controversies," 121.
[91] Rob Iliffe, "Butter for Parsnips: Authorship, Audience, and the Incomprehensibility of the *Principia,*" in *Scientific Authorship: Credit and Intellectual Property in Science,* edited by Mario Biagioli and Peter Galison (New York: Routledge, 2003), 37.
[92] See esp. Alan Shapiro, *Fits, Passions, and Paroxysms* and "Experiments and Mathematics" and Niccoló Guiciardini, *Isaac Newton on Mathematical Certainty and Method* (Cambridge, MA: The MIT Press, 2009).

central figure of eighteenth century natural philosophy, it is essential to integrate him more successfully into the historiography of the scientific method. It was not that Newton was unable to understand the arguments made by Hooke, nor that Hooke was unable to understand Newton, but that they viewed natural philosophy quite differently and were trying to accomplish incompatible things. Hooke was seeking to defend his hypothesis, albeit aggressively, and to modify Newton's dogmatic tone. Newton, however, had intentionally expressed himself in such a manner because he was trying to reconstruct how certainty was conceived by the experimental community. Therefore, neither Hooke nor Newton could have possibly accepted the other's arguments. To do so would have been to admit the entire foundation for their systems of natural philosophy were flawed.

When the dispute is re-framed through consideration of literary technology a strong argument can be made that Newton knew exactly what the import of the dispute was. It was neither a matter of Newton being unable to grasp the arguments of his opponents nor his opponents being unable to understand the depth of Newton's theory. Instead, Newton was consciously attempting to assert his new model of natural philosophy, while his opponents sought to defend the status quo. It is not that he was ignorant of or unable to understand the Boylean position, but that he was trying to reconstruct natural philosophy in accordance with his 'doctrine.' Where Bechler has interpreted Newton as failing to comprehend the argument of his opponents, I would suggest that Newton was instead seeking to steer the conversation away from Boylean language and back toward the mathematical principles he had proposed in his original paper.

This chapter has sought to provide an overview of the dispute between Hooke and Newton. As well, I have aimed to demonstrate the unique aspects of this particular controversy in comparison to the others in which Newton had found himself following the publication of the "New theory" in order to explain why I have focused my study on the single dispute between Hooke and Newton. In the second chapter, I will turn from the more straightforward narrative that has been given thus far to delve more deeply into an analysis of Newton's literary technology. Part of the reason why Newton's theory caused so much controversy was that he had blithely promoted as self-evident when, as A. Rupert Hall has noted, Newton "had gradually accustomed himself to a new language of optical theorization, which his critical readers failed to understand."[93] Newton's theory was based on what was essentially an intuitive leap. Newton was seeking to move natural philosophy toward what Peter Dear has called "physico-mathematical philosophy" or "mixed mathematics."[94] Therefore, I will investigate more broadly the philosophical context in which Newton was working and provide a more comprehensive analysis of Newton's literary technology.

[93] Hall, *Isaac Newton,* 102.
[94] Dear, *Discipline and Experience.*

Chapter Two
"Not an Hypothesis but most rigid consequence": Isaac Newton's Literary Technology

> "Pavlov believed that the ideal, the end we all struggle toward in science, is the true mechanical explanation. He was realistic enough not to expect it in his lifetime. Or in several lifetimes more. But his hope was for a long chain of better and better approximations. His faith ultimately lay in a pure physiological basis for the life of the psyche. No effect without cause, and a clear train of linkages."
> – Thomas Pynchon, *Gravity's Rainbow*

A favorite apocryphal story for historians of Newton is that of the witty Cambridge undergraduate who, upon seeing Newton walk past, quipped, "there goes the man who wrote the book that neither he nor anyone else can understand." The *Principia* outlined Newton's theory of gravitation in the most stark, mathematical terms imaginable. He refused to make any concessions to his readers and instead sought to address an audience limited to the most advanced mathematicians in Europe. This was a conscious response by Newton to the controversies that the "New theory about light and colour" had engendered. As Rob Iliffe has argued, the obscurity of the *Principia* was part of Newton's strategy for self-fashioning and was intended to secure his authority.[1] It was difficult for opponents to develop detailed criticisms of a theory they lacked the necessary skill to comprehend. However, Newton's process of self-fashioning began much earlier than the 1680s. Iliffe concentrated on the means by which Newton attempted to construct his public persona and not on the rhetorical strategies he adopted in his writing to secure his credibility. Furthermore, Iliffe has focused primarily on the period of the *Principia,*

[1] Rob Iliffe, "'Is He Like Other Men?' The Meaning of the *Principia Mathematica*, and the Author as Idol," in *Culture and Society in the Stuart Restoration: Literature, Drama, History*, edited by Gerald Maclean (Cambridge: Cambridge University Press, 1995).

therefore, he has failed to give full weight to the significance of the “New theory” to the development of Newton's literary technology.

The “New theory” demonstrates some of the ways in which dissemination is at the heart of science. In the words of Bruno Latour, “To convince someone that an experiment has succeeded, that a technique is effective, that a proof is decisive, there must be *more than one actor*.”[2] Latour has rejected what he called the 'diffusionist' model; one that attributed the “power to revolutionize society” to individual genius.[3] The argument upon which this thesis rests is that science is a collaborative enterprise. There is no such thing as the solitary genius. Fundamental to dissemination is the scientific publication. In this chapter I look to address the issue of Isaac Newton’s literary technology in order to define his scientific method, how it fit in comparison to his contemporaries Robert Boyle and Robert Hooke. In doing so I suggest key ways in which Newton’s approach developed and changed over time, consequently shaping eighteenth century natural philosophy.

There has been no shortage of publications on the subject of science in the public sphere; however, it has tended to regard the public sphere in terms of display or commodification. The more rare studies that focus on publication fail to do so in a manner that recognizes the public sphere or do so in a way that is too limited. Peter Dear has argued that “an account of an experiment is an inseparable part of its meaning.”[4] Indeed, “An experiment, therefore, is only an experiment if it appears as one in scientific

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[2] Bruno Latour, *The Pasteurization of France*, translated by Alan Sheridan and John Law (Cambridge, MA: Havard University Press, 1988), 15. Emphasis in original.
[3] Latour, *Pasteurization*, 113-4.
[4] Peter Dear, “Narratives, Anecdotes and Experiments,” in *Literary Structure of Scientific Arguments: Historical Studies*, edited by Peter Dear (Philadelphia: University of Pennsylvania Press, 1991), 136.

discourse, or might well do so given the context in which it was created."[5] Steven Shapin, meanwhile, has suggested three separate, but overlapping or "embedded," technologies in his analysis of Robert Boyle's air-pump experiments. These technologies were: material ("embedded in the construction and operation of the air-pump"); literary ("by means of which the phenomena produced by the pump were made known to those who were not direct witnesses"); and social ("which laid down the conventions natural philosophers should employ in dealing with each other and considering knowledge-claims").[6] In this chapter I will expand on his concept of 'literary technology,' which Shapin defined as "the expository means by which matters of fact were established and assent mobilized."[7]

The production of knowledge and its communication are not the distinct activities they are usually regarded to be; instead, "speech about reality is a means of generating knowledge about reality, of securing assent to that knowledge, and of bounding domains of certain knowledge from areas of less certain standing."[8] They are "crafted to make representations of the natural and social worlds stand independently of their authors."[9] In studying literary technology, "we are not, therefore, talking about something which is merely a 'report' of what was done elsewhere; we are dealing with a most important form

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[5] Dear, "Narratives," 137. See also Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, MA: Harvard University Press, 1987), 40.
6 Steven Shapin, "Pump and Circumstances: Robert Boyle's Literary Technology," *Social Studies of Science* 14 (1984), 484.
[7] Shapin, "Pump and Circumstance," 484; see also Jan Golinski, "Robert Boyle: Scepticism and Authority in Seventeenth-Century Chemical Discourse," in *The Figural and the Literal: Problems of Language in the History of Science and Philosophy,* edited by Andrew E. Benjmin, Geoffrey Cantor and John R.R. Christie (Manchester: Manchester University Press, 1987).
[8] Shapin, "Pump and Circumstances," 481.
[9] Simon Schaffer, "The Leviathan of Parsontown: Literary Technology and Scientific Representation," in *Inscribing Science: Scientific Texts and the Materiality of Communication,* edited by Timothy Lenoir (Stanford: Stanford University Press, 1998), 183.

of experience and means for extending and validating experience."[10] Shapin's work on literary technology has focused primarily on Robert Boyle to whom, I argue, Newton was consciously responding when he sought to develop his own literary technology. Thus, it is necessary to begin by describing Boyle's model.

Boyle's papers were always left to be published in a state of chaos and incompleteness; a fact for which he constantly apologized.[11] *Touching Colours,* he explained, was "written to a private Friend" and was done so "by snatches, at several times, and places" meaning he often did not have with him what he had already written.[12] Shapin has argued that such language by Boyle was not merely a reflection of disorganization, but was part of a conscious strategy for credibility.[13] Jan Golinski has contended that Boyle intended to establish authoritative foundations for subsequent discourse on chemistry.[14] Boyle's "meta-discourse," to borrow John Harwood's term, established an aura of authenticity and immediacy.[15] His experiments were to be seen as having been actually performed and recorded without editorializing by Boyle as they had occurred.[16]

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[10] Shapin, "Pump and Circumstances," 484.
[11] See Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago:University of Chicago Press, 1994), 178.
[12] Robert Boyle, "Experiments and Considerations Touching Colours (1664)", in *The Works of Robert Boyle, Electronic Edition, vol. IV: Colours and Cold, 1664-5,* edited by Michael Hunter and Edward B. Davis (Charlottesville, VA: InterLex Corporation, 2003), 5.
[13] Shapin, "Pump and Circumstance."
[14] Jan Golinski, "Robert Boyle," in *The Figural and the Literal*.
[15] John Harwood, "Science Writing and Writing Science: Boyle and Rhetorical Theory," in *Robert Boyle Reconsidered*, edited by Michael Hunter (Cambridge: Cambridge University Press, 1994), 42.
[16] Boyle was particularly critical of fictive experiments or thought experiments, especially when presented as if they were real. See Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995), Ch. 7. For Boyle's criticism regarding Blaise Pascal's experiments see Boyle, "Hydrostatic Paradoxes, Made Out by New Experiments," in *Works, vol. V.* For a more general discussion on the subject by Boyle see *The Sceptical Chymist*, in *Works, vol. II,* 209.

The development of Boyle's literary technology has a good deal in common with similar changes to how history was being written in the seventeenth century. Barbara Shapiro has argued that there was a correlation between early modern historical writing and natural philosophy. Historians were engaged in debate as to whether they should provide only a straightforward narrative of facts or if they "were obligated to consider the causes and explanations." The general trend by the seventeenth century, Shapiro has argued, was to prefer "'bare narration' of 'fact.'"[17] Boyle viewed his experimental reports to be histories of discrete events. It is perhaps, therefore, unsurprising that Boyle adopted historical conventions in his writings. In doing so Boyle challenged the common view that philosophy was intrinsically about causal explanations. While the eventual goal of the new science was to uncover causes, he did not believe that enough was known about the natural world for universal theories or philosophic systems to be meaningfully developed.

For an analysis of Boyle's literary technology, Steven Shapin's model will be adopted.[18] The central tenet of Shapin's thesis is that of the role of witnessing. Shapin and Simon Schaffer coined the term "virtual witnessing" to describe the process by which the witnessing experience was "multiplied."[19] The empirical, experiment-driven natural philosophy of Robert Boyle required a technology that ensured "the things had been done

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See also Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 44.

[17] Barbara Shapiro, *A Culture of Fact: England, 1550-1720* (Ithaca: Cornell University Press, 2000), 53.

[18] For criticisms of Shapin see Rose-Mary Sargent, *A Diffident Naturalist: Robert Boyle and the Philosophy of Experiment* (Chicago: University of Chicago Press, 1995); Michael Hunter, ed. *Robert Boyle Reconsidered* (Cambridge: Cambridge University Press, 1994).

[19] Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985), 60-5; Cf. Shapin, "Pump and Circumstance," 483-4.

and done in the way claimed."[20] Such a technology was particularly important in the case of Boyle's air-pump experiments because the air-pump was both notoriously difficult to operate and expensive to build, meaning that direct access to it was highly limited. Indeed, there likely was only seven air-pumps in existence in the mid-1660s.[21] Therefore, Boyle needed to develop a reliable means to establish the authority of his matters of fact without the expectation of his readers being able to replicate or directly witness his experiments.

Virtual witnessing was intended to overcome the problem of access by recreating the experiment via literary means. Thus, according to Shapin and Schaffer, Boyle's notorious verbosity was an intentional strategy. Their argument has, however, been criticized by Michael Hunter as presenting a picture of Boyle as being far more in control of his "personality development" than Hunter has deemed appropriate.[22] While it was likely true that Boyle's literary technology and scientific method was shaped partly by his personality rather than being an entirely conscious stratagem, it would be unfair to Boyle not to recognize that he did deliberately shape his method in order to enhance his authority. Boyle's publications are perhaps most notable for the disorder of their presentation and the exhaustive details included in the narrative, both of which had specific rhetorical advantages. The benefit of his approach was that it leant greater credibility to Boyle as having reported on his experiments as they happened rather than having edited them into a fictive narrative constructed *post facto* to prove his conclusions.

[20] Shapin and Schaffer, 60.
[21] Shapin, "Pump and Circumstance," 487-8.
[22] Michael Hunter, *Boyle: Between God and Science* (New Haven: Yale University Press, 2009), 261.

Boyle went so far as to include failed experiments in order to demonstrate that he was providing his readers with the unabridged truth.[23] To quote Boyle, "I think it becomes one that professes himself a faithful Relator of Experiments, not to conceal" experimental failures.[24] Of course, in practice Boyle was highly selective in his use of such failures.

Boyle's defence of including failed experiments came in conjunction with one such notable experiment. In the process of conducting his air-pump trials, he had developed a theory of 'cohesion' relating to the idea that if two well-polished marble discs were laid on each other they would "stick so fast together" that when one lifted the top disc the bottom would be lifted as well.[25] Boyle speculated:

> A probable cause of this so close adhesion we have elsewhere endeavour'd to deduce from the unequall pressure of the Air upon the undermost stone; For the lower *superficies* of that stone being freely expos'd to the Air is press'd upon by it, whereas the uppermost surface, being contiguous to the superiour stone, is thereby defended from the pressure of the Air which consequently pressing the lower stone against the upper, / hinders it from falling, as we have elsewhere more fully declar'd. Upon these grounds we conjectur'd that in case we could procure two marbles exactly ground to one another; and in case we could also sufficiently evacuate our Receiver, the lower stone would, for want of the wonted and sustaining pressure of the Air, fall from the upper.[26]

However, Boyle was unable to carry out an experiment that demonstrated his hypothesis. He found that he could not obtain marbles smooth enough that they would stay together for more than a few minutes. When he was finally able to get them to stick together, the marbles failed to separate the way his theory argued that they would.[27] Because the pump

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[23] Shapin and Schaffer, 64-5.
[24] Boyle, "New Experiments Physico-Mechanical, Touching the Spring of Air and Its Effects (1660)," in *Works, vol. I,* 183.
[25] Shapin and Schaffer, 47; see Experiment 31 in Boyle, "New Experiments," in *Works, vol. I,* 238-40.
[26] Boyle, "New Experiments," in *Works, vol. I,* 238.
[27] Shapin and Schaffer, 48.

leaked, however, Boyle was able to contend the experiment be accounted unsuccessful without abandoning the hypothesis itself. By including such accounts of experimental failure, Boyle thereby constructed a literary identity of himself as credible, trustworthy and modest. The implication was that his matters of fact should be accepted by his audience because if there was a credible reason why they should not have been he would have given it in the narrative.

A further example of Boyle's literary technology in action can be found with his *Experiments and Considerations Touching Colours* (1664). Unlike his experiment with marbles, Boyle was careful to present his work on colours without theory. It was the:

> Design of this Treatise is to deliver things rather *Historical* than *Dogmatical*, and consequently if I have added divers new *speculative* Considerations and hints, which perhaps may afford no despicable Assistance towards the framing of a solid and comprehensive Hypothesis.[28]

He was providing his readers with a report of experiments carried out from which they would be able to begin to draw some conclusions regarding the nature of colours. Rather than a "compleat Fabrick, or so much as Modell," Boyle intended "only to bring in Materials proper for the Building" of the experiments.[29] This was consistent with Boyle's opposition to 'universal' theories, which he viewed as overly dogmatic and tantamount to 'Cartesian' systems building.[30]

Touching Colours was emblematic of Boyle's modest method. He opened with a string of apologies for the state of the manuscript. The experiments could have been

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[28] Boyle, "Experiments and Considerations Touching Colours," in *Works, vol. IV,* 5.
[29] Boyle, "Touching Colours," in *Works, vol. IV,* 6.
[30] Shapin, *Social History of Truth,* 317.

"deliver'd in fewer words." It was "written to a private Friend" in "snatches." The experiments were not arranged to their best advantage, therefore, "some connections and consecutions of them might easily have been mended." Having conducted the experiments over an extended period of time and in various locations, some of his notes had become misplaced. Boyle did not have the luxury of time, he argued, to organize and prepare his experiments for publication to the degree he would have liked.[31] He hoped that he would be "excused by those that both know, how nice divers experiments of Colours are" if he were "to insist long upon the circumstances of a Tryall" as he "was not barely to *relate* them, but so as to teach a young Gentleman to make them."[32] The new science, after all, depended on the notion of replication and accessibility; therefore, it was essential that experiments be described in a way conducive to reproduction. Lastly, he apologized for not being more "solicitous" to divide his treatise more nicely than into three parts. Boyle "contented" himself "with this easie Division of my Discourse" as he "did not think it so necessary to be Curious about the Method or Contrivance of a Treatise."[33]

In *Touching Colours* Boyle sought primarily to describe the experiments he had carried out. The aim was not to give his reader a completed theory of colour, but to demonstrate some of the properties possessed by colours and to show a number of interesting phenomena relating to them. That Newton was inspired to take up the study of colours after reading *Touching Colours* is evidence that Boyle succeeded in his purpose.

[31] Boyle, "Touching Colours," in *Works, vol. IV,* 5.
[32] Boyle, "Touching Colours," in *Works, vol. IV,* 6.
[33] Boyle, "Touching Colours," in *Works, vol. IV,* 6.

The intent of Boyle's narrative was to describe discrete events as they had occurred in terms that would allow his readers to understand how the experiment worked so that they would be able to repeat it should they wish. Of course, in the case of his air-pump experiments such replication was not readily possible for most due to the limited access to air-pumps. Shapin and Schaffer have argued that Boyle sought to overcome this problem through the exhaustiveness of his account, which they termed "virtual witnessing."[34] Boyle's style was intended to involve "the production in a *reader's* mind of such an image of an experimental scene as obviates the necessity for either direct witness or replication."[35]

The inclusion of a complete narrative that often went well beyond the specifics of the experiment itself to include general information regarding the context, setting and environment in which the experiment had occurred not only gave greater authenticity to the account—it extended the witnessing process. Boyle obsessively included "circumstantial details" in his reports. This was because, as Peter Dear has observed, "The procedure could always be repeated; the event could never be."[36] Baconian natural philosophers such as Boyle "scorned thought experiments and insisted upon both accurate and circumstantial reporting."[37] He was troubled by those who published and built upon "Chymical Experiments, which questionless they never try'd" who "have been content rather to beleeve what they so boldly Affirm, then be at the trouble and charge, to try

[34] Shapin and Schaffer, 60-1.
[35] Shapin and Schaffer, 60.
[36] Peter Dear, *"Toitus in Verba*: Rhetoric and Authority in the Early Royal Society, *Isis* 76 (1985): 153.
[37] Kuhn, *Essential Tension*, 44.

whether or no it be True."[38] For instance, Boyle pointed to Blaise Pascal's treatise "On the aequilibrium of liquors" in which Pascal had reported trials that he "could not have performed" because they required instruments of an exactness beyond what a tradesman would have been able to obtain.[39] Experiments needed to be actually performed because:

> Experiments that are but speculatively true, should be propos'd as such, and may oftentimes fail in practise; because there may intervene divers / other things capable of making them miscarry, which are overlook'd by the Speculator, that is wont to compute only the consequences of that particular thing which he principally considers.[40]

Furthermore, Boyle's detailed descriptions served to remove judgment from his accounts. He was reporting matters of fact and not constructing experiments to demonstrate a preconceived hypothesis. Boyle sought to make no distinction between "natural history" and the "experimental;" because "each was, in the same way, given as an experience defined in space and time by an actor, the observer."[41] Boylean natural philosophy was rooted in the discrete event.

Robert Hooke had a unique view of the process. He was Boyle's assistant during the period when the air-pump experiments were carried out. When he later criticized Newton's "New theory," Hooke did so using language clearly influence by Boylean ideology. Hooke, however, did not have Boyle's inherent advantage of status. Indeed, Hooke's contemporaries did not regard him as a natural philosopher, but instead as a "mechanic."[42] Therefore, it would also seem likely that he would have had to adjust his

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[38] Boyle, "Sceptical Chymist," in *Works*, 209; 277.
[39] Sargent, "Learning from Experience," 61.
[40] Boyle, "Hydrostatical Paradoxes," *Works, vol. V*, 225.
[41] Dear, "*Toitus in Verba*," 154.
[42] Adrian Johns, *The Nature of the Book: Print and Knowledge in the Making* (Chicago: University of Chicago Press) , 470.

method in order to overcome this 'deficiency. While Hooke had an obvious weakness compared to Boyle, he also had an advantage over Newton. Hooke held the position of curator of experiments for the Royal Society, thus he was in the position of publicly performing experiments before an elite audience. As Steven Shapin has observed, “Hooke lived on a public stage.”[43] Though Hooke maintained his personal credibility with fellow members of the Royal Society by regularly demonstrating experiments to them, the Society’s leadership was “increasingly aware of print as a way to establish, enhance, and protect its public image.”[44] It was for such ends that Hooke wrote *Micrographia* (1665).

Steven Shapin has regarded science as consisting of three embedded technologies. Along with literary technology he included material technology, which embedded the construction and operation of experimental instruments and social technology, which “laid down the conventions natural philosophers should employ when dealing with each other and considering knowledge claims.”[45] While Newton relied on his audience accepting his written account, to a large extent Hooke’s authority depended very little on his written work; instead, he primarily employed social technology.[46] In contrast to Newton, Hooke “linked the issue of credibility to the rhetorical utility of statements that

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[43] Steven Shapin, “The House of Experiment in Seventeenth-Century England,” *Isis* (1988): 382.
[44] John Harwood, “Rhetoric and Graphics in *Micrographia*,” in *Robert Hooke: New Studies*, edited by Michael Hunter and Simon Schaffer (Woodbridge, UK: The Boydell Press, 1989), 128.
[45] Shapin, “Pump and Circumstances,” 484.
[46] Iwan Rhys Morus has provided a useful discussion of public demonstration, self-fashioning and social technology using the example of Michael Faraday, see Morus, *Frankenstein’s Children: Electricity, Exhibition, and Experiment in Early-Nineteenth-Century London* (Princeton: Princeton University Press, 1998), Ch. 1.

showed, rather than told, the scientific facts."[47] While he primarily depended on social and material technology for his credibility, the three technologies cannot be seen as distinct; instead, "each embedded the others."[48] Indeed, with Hooke the three technologies overlapped much more so than was the case with Newton as Hook's treatise included substantial and carefully crafted illustration intended to visually demonstrate his observations.[49]

Considering my focus on Hooke's response to Newton's method and language, it is important to examine briefly Hooke's literary technology. In this section, therefore, I will look at Hooke's approach in *Micrograph*. As Hooke triumphantly proclaimed, with telescopes and microscopes, nothing was "so far distant" or "too small" to "escape inquiry."[50] Thus, much like Boyle's experiments on colour and light, Hooke primarily sought to introduce his audience to new phenomena. At the same time, however, Hooke was far more willing to proffer explanatory theory than Boyle had been. Hooke tried to distinguish between his "methodological accomplishments," which he saw as belonging "to the community" and his "conjectures" that "were his own."[51] In doing so he provided new matters of fact while at the same time he was able to assert a causal theory without presenting a doctrine which was overly dogmatic.

Micrographia contained a total of sixty "observations" made by Hooke using optical instruments. As Hooke argued during his dispute with Newton, it was essential

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[47] Larry Stewart, *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992), 13.
[48] Shapin, "Pump and Circumstances," 484.
[49] Harwood, "Rhetoric and Graphics," 121.
[50] Robert Hooke, *Micrographia, or, Some physiological descriptions of minute bodies made by magnifying glasses with observations and inquiries thereupon* (London: Jo. Martyn and Ja. Allestry, 1665), iv.
[51] Shapin and Schaffer, 321-2.

that a large body of experiments be performed and made available to the audience.[52] The experiments given in *Micrographia* were a diverse product of the eclectic nature of Hooke's interests. From a Boylean point-of-view, however, this eclecticism was a virtue. For Boyle, the primary role of natural philosophers was to build up a large body of experimental observations about nature. Boyle's method “inverted” the order of discovery and proof held by “traditional” philosophers who began with speculations about universal causes. While “for most of the century and in most places...reliance on isolated testimony was irrelevant because prepackaged philosophical universality was the norm,” Boyle argued that philosophers should “first compile a vast amount of information about natural effects in order to discover 'how things have been or are really produced.'”[53] To begin from universals would inherently be to bias the conclusions. This approach was influenced by the ancient distinction between “historia,” which “dealt with particulars” and “philosophy,” which dealt with “universals.”[54] As Hooke limited *Micrographia* to sixty observations, it is clear that it was the dogmatic priority given to the crucial experiment by Newton to which Hooke objected and not the fact that Newton failed to include a description of every experiment he had carried out. It was appropriate to abridge the description into something manageable as long as the reader was given a broad enough sample of experiments and the conclusions were not prejudiced by the elevation of one experiment over the others.

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[52] Hooke to Oldenburg, 15 February, 1671/2, *Correspondence, vol. I,* 110-1.

[53] Dear, *Discipline and Experience,* 149; Rose-Mary Sargent, “Learning From Experience: Boyle’s Construction of an Experimental Philosophy,” in *Robert Boyle Reconsidered,* edited by Michael Hunter (Cambridge: Cambridge University Press, 1994), 68.

[54] Barbara Shapiro, *Culture of Fact,* 54.

Isaac Newton consciously set out to develop a literary technology of his own with which he would be able exhibit a higher degree of certainty for his claims than was deemed acceptable by Boyle. The mathematical tradition Newton looked to link with experimental science stressed the generality of experiment and sought to place it within a carefully articulated theory.[55] English natural philosophers "had abandoned the demand for certain scientific knowledge and developed the alternative concept of probable knowledge."[56] Indeed, this lack of certain knowledge was Thomas Hobbes' primary criticism of Boyle's method.[57] Boyle, however, did not regard the "probabilistic conception of physical knowledge" to have been "a regrettable retreat from more ambitious goals"; instead, it was a "wise rejection of failed dogmatism."[58] Newton conspicuously took a position that was at odds with this view and rejected that whatever was explained in philosophy were hypotheses that differed only in degree of probability. What Newton was attempting to do was develop an experimental approach that could both attain the level of certainty possible in mathematics and remain firmly located within the experimental context. He could not abide by a philosophy that rendered matters of fact to be only probably true, at the same time; however, he sharply criticized Gottfried Leibniz in 1715 for preferring "hypotheses to arguments of induction drawn from experiments."[59] Newton had concluded when he composed the "New theory," that the only ways to publish without causing "unnecessary dispute" were to use a

[55] Alan Shapiro, *Fits, Passions and Paroxysms*, 36.
[56] Alan Shapiro, *Fits, Passions and Paroxysms*, 19.
[57] Mary Poovey, *A History of the Modern Fact: Problems of Knowledge in the Sciences of Wealth and Society* (Chicago: University of Chicago Press, 1998), 104.
[58] Shapin, "Pump and Circumstances," 483.
[59] Newton quoted in Alexander Koyè and I. Bernard Cohen, "The Case of the Missing Tanquam: Leibniz, Newton & Clarke," *Isis* 52 (1961), 558.

mathematical format and to treat a ray of light as "an abstract mathematical entity defined merely by its degree of refrangibility."[60] It was not a matter of Newton failing to follow protocol; instead, he was actively seeking to re-orient natural philosophy in order to obtain greater certainty.

While Shapin regarded Boyle as having "exhibited the proper means by which legitimate knowledge was to be generated and evaluated," Shapin's analysis cannot be applied equally to Newton.[61] According to Shapin, Boyle "did not *take on* the identity of experimental philosopher, he was a major force in *making* that identity."[62] However, Newton quickly supplanted Boyle's position as the embodiment of the experimental natural philosopher and Newton's authority defined much of the eighteenth century. Shapin's analysis only works for natural philosophy "that rested on discrete, singular experiences presented historically"—the kind of experimental philosophy promoted by Boyle.[63] Furthermore, Shapin presumed a natural philosophical community composed entirely of gentleman virtuosos. As Peter Dear has contended, "Boylean experimental philosophy was not the high road to modern experimentalism; it was a detour."[64] Boyle did not represent the typical natural philosopher and possessed personal characteristics, particularly in terms of his social status, that made him an anomalous figure in experimental philosophy after the 1660s. As such, I look to expand on Dear's argument that "Newton's work retrospectively validated the experimental program that Boyle had

[60] Rob Iliffe, "Butter for Parsnips: Authorship, Audience, and the Incomprehensibility of the *Principia*," in *Scientific Authorship: Credit and Intellectual Property in Science,* edited by Mario Biagioli and Peter Galison (New York: Routledge, 2003), 41.
[61] Shapin, "Pump and Circumstance," 482.
[62] Shapin, "*Social History of Truth*," 126-7.
[63] Dear, *Discipline and Experience,* 149.
[64] Dear, *Discipline and Experience,* 3.

advocated," reconstituting it in mathematical terms, thereby giving English experimental philosophy "a way of representing their activity as experimentally meaningful."[65] By examining Newton's early efforts at developing his own literary technology, I show the way in which he was actively seeking to re-orient natural philosophy in a way that would allow it to adopt mathematical certainty and philosophic universality while maintaining the concrete reality of the discrete event.

It is typical to characterize Newton as having been disinterested in public approval. On a superficial level, his reluctance to publish or make his theories widely known would seem to give credence to such a view. He certainly did not accord witnessing with anything like the same significance as Boyle did. Indeed, "rather than to any credible gentlemen who might be able to vouch for his claims," Newton's correspondence referred "to a stock of private treatises and mathematical discoveries."[66] He was of the view that "What's done before many witnesses is seldome without some further concern then that for truth;" instead, "what passes between friends in private usually deserves ye name of consultation rather then contest."[67] Rather than public disputes, Newton requested that "If there be any thing els in my papers in wch [Hooke] apprehend I have assumed too much, or not done you right, if you please to reserve your sentiments of it to private letter." Newton promised that if Hooke did so he would find Newton not to be "so much in love wth philosophical productions but yt I can make them yeild to equity & friendship."[68]

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[65] Dear, *Discipline and Experience*, 242; 8.
[66] Iliffe, "Is He Like Other Men?" 161.
[67] Newton to Hooke, 5 February 1675/6, *Correspondence, vol. I,* 416.
[68] Newton to Hooke, 5 February 1675/6, *Correspondence, vol. I,* 416.

Isaac Newton, ultimately, was a mathematician. Alan Shapiro has argued that Newton developed his *Lectiones opticae* in order to construct a mathematical model of colour.[69] The optical lectures served to provide the large experimental basis for the conclusions Newton presented in the "New theory." Newton began by defending his treatment of colour mathematically saying:

> But lest I seem to have exceeded the bounds of my position while I undertake to treat the nature of colors, which are thought not to pertain to mathematics, it will not be useless if I again recall the reason for this pursuit. The relation between the properties of refractions and those of colors is certainly so great that they cannot be explained separately. Whoever wishes to investigate either one properly must necessarily investigate the other. Moreover, if I were not discussing refractions, my investigation of them would not then be responsible for my undertaking to explain colors; nevertheless, the generation of colors includes so much evidence, that for their sake I can thus attempt to extend the bounds of mathematics somewhat, just as astronomy, geography, navigation, optics, and mechanics are truly considered mathematical sciences even if they deal with physical things...although colors may belong to physics, the science of them must nevertheless be considered mathematical...I hope to show—as it were, by my example—how valuable mathematics is in natural philosophy.[70]

Newton finally gave up on a mathematical explanation of colour, attempted to suppress the lectures and in *Opticks* (1704) he only hinted at the scheme. Indeed, the bold assertions of the "New theory" were reduced to "My design in this Book is not to explain the Properties of Light by Hypothesis, but to propose and prove them by Reason and Experiment."[71]

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[69] Alan Shapiro, "Experiment and mathematics," 36.
[70] Newton, *The Optical Papers of Isaac Newton, vol. I: The Optical Lectures,* translated and edited by Alan Shapiro (Cambridge: Cambridge University Press, 1984), 439.
[71] Newton, *Opticks, or, A Treatise of the Reflections, Refractions, Inflections & Colours of Light* (New York: Dover Publications, 1952), 1.

Thomas Kuhn has contended that there were two distinct lines of Newtonian influence, one that can be traced from *Principia* and the other from *Opticks*. The suggestion being that the *Principia* is mathematical, while *Opticks* experimental. As Kuhn has explained, classical sciences were grouped together as mathematical while Baconian science was viewed as experimental.[72] The primary idea suggested by Kuhn was to enforce a distinction between so-called classical sciences and Baconian sciences and to emphasize an English exceptionalism, which saw it rapidly adopt Baconian and utilitarian science at the expense of the mathematical. This notion of a distinct division between English and continental philosophy, as described by Kuhn, is overly simplistic as has been demonstrated by Peter Dear in his discussion of mixed mathematics.

An example Dear used to show the difference between continental natural philosophy and that of the Royal Society was Blaise Pascal and the famed ascent of Puy-de-Dôme by Pascal's brother-in-law Périer. This experiment involved taking barometer measurements at increasing elevations in order to show the correlation between altitude and air pressure.[73] While Boyle strongly criticized Pascal for insufficiently establishing that his experiments were actually carried out, Dear has argued that Boyle failed to understand the paradigm in which Pascal operated. Baconian science was concerned with singular experience. In contrast, for Pascal and his continental contemporaries, natural philosophy "took the form of universal statements" from which specific ideas about the

[72] Kuhn, *Essential Tension,* 48.
[73] Dear, *Discipline and Experience,* Ch. 7.

world could be derived.[74] The Boylean notion of the singular experience was to rely on fallible historical reports that "were not public, but known only to a privileged few."[75]

While Boyle eschewed the use of mathematics in natural philosophy, for philosophers such as Pascal, "mathematics appeared as one of the few refuges of eternal verity untainted by the possibility of dissent, while all around them the natural world displayed a variety of impenetrability that mocked attempts at framing it."[76] Interestingly, this same recognition of the variety of the natural world was seen by Boyle as an argument in favour of Baconian science.[77] The kind of "historical report[s] of a specific event" about which "Boyle wrote endlessly, would have been scientifically meaningless" to Pascal.[78] Unlike Kuhn, however, Dear did not make a complete distinction between continental philosophy and English philosophy. He has traced a direct link between the mathematical philosophy as practiced by Pascal with the mixed mathematics of Isaac Newton. A key link was John Wilkins who "hoped to spread the new mathematical approach to mechanics to the general public."[79] According to Dear, it was the "mathematical argument in making accredited knowledge of nature show how the foundational assumptions of a mathematical science, and the kinds of experiences that

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[74] Dear, *Discipline and Experience,* 44.
[75] Dear, *Discipline and Experience,* 44.
[76] Shapin, *Social History of Truth,* 315-7; Dear, *Discipline and Experience*, 11.
[77] See Lorraine Daston, "Marvelous Facts and Miraculous Evidence in Early Modern Europe," *Critical Inquiry* 18 (1991): 93-124.
[78] Dear, *Discipline and Experience*, 208-9.
[79] Barbara Shapiro, *John Wilkins, 1614-1672: An Intellectual Biography* (Berkely: University of California Press, 1969), 43.

underwrote them" that "made it possible for Newton to announce a kind of declaration of independence for physico-mathematics."[80]

Boyle extrapolated in detail on his experiments and methodology and included descriptions of failed experiments in order to enhance his credibility. Newton took essentially the opposite approach. Newton tried to purge from his writing all conjecture and information that he deemed not to be experimentally certain or relevant.[81] He insisted that experimental philosophers "not mingle conjectures with certainties."[82] Thus, he drew a sharp distinction between hypothesis and experimental facts. Such a contention that hypotheses not be mingled with demonstrated principles led to the presentation of his theory in a "stark, nearly unintelligible form" that generated confusion for his readers.[83] He responded indignantly to Pardies that:

> I am content that the Reverend Father calls my theory an hypothesis if it has not yet been proved to his satisfaction. But my design was quite different, and it seems to contain nothing else than certain properties of light which, now discovered, I think are not difficult to prove, and which if I did not know to be true, I should prefer to reject as vain and empty speculation, than acknowledge them as my hypothesis.[84]

For Newton, his 'theory' was better than Hooke's 'hypothesis' because Hooke's explanation lacked sufficient variables to account for all of the observed phenomena, a flaw that Newton asserted his own theory did not share.[85] In his writing Boyle had sought to demonstrate his credibility and authority through the provision of detailed narratives.

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[80] Dear, *Discipline and Experience,* 210.
[81] Alan Shapiro, "Huygens' 'Traité de la Lumière' and Newton's 'Opticks': Pursuing and Eschewing Hypotheses," *Notes and Records of the Royal Society of London* 43 (1989): 228. Newton, however, was not always able to resist conjecture, for an example see Newton, *Optical Papers, vol I,* Lectures II, 14-5, 132-5.
[82] Newton to Oldenburg, 6 February, 1671/2, *Correspondence, vol. I,* 100.
[83] Alan Shapiro, *Fits, Passions and Paroxysms,* 4.
[84] Newton to Oldenburg, 13 April, 1672, *Correspondence, vol. I,* 144.
[85] Alan Shapiro, *Fits, Passions and Paroxysms,* 52.

Newton, in contrast, based his authority upon the mathematical certainty of his theory and the careful separation of the experimentally proven from the hypothesis. Zev Bechler has further pointed to the bluntness of Newton's style as being intentional. Newton's first response to Hooke was one of four drafts that he composed. The first draft was three times the length of that which was ultimately published and "except for some minor improvements, he aimed mainly to sharpen the bite and aggressiveness of his style."[86]

Newton's experimental facts themselves went largely unchallenged in England. Henry Guerlac has suggested that this was because Hooke had tried and accepted them as valid.[87] This was not the case on the continent where Newton's crucial experiment remained an object of dispute well into the eighteenth century. Initially, the problem was that Newton's description of the experiment was too obscure and he had failed to provide sufficient details as to how monochromatic rays were produced or "how to prove that color and refrangibility are indissolubly linked."[88] Even with the expanded explanations provided by Newton in his correspondence during the disputes in the 1670s and again in *Opticks,* the experiment remained problematic because it did not actually work exactly as Newton claimed. This was 'proven' conclusively by Edme Marriotte's attempt at replicating Newton's experiment. According to Guerlac, Marriotte "found the emerging violet rays tinged with red and yellow." Marriotte concluded the rays "had been altered or 'modified' by their passage through the second prism" and it was "evident" that "a given portion of light receives different colors as a result of different modifications, and that the

[86] Zev Bechler, "'A Less Agreeable Matter': The Disagreeable Case of Newton and Achromatic Refraction," *The British Journal for the History of Science* 8 (1975 111.
[87] Henry Guerlac, *Newton on the Continent* (Ithaca: Cornell University Press, 1981), 88.
[88] Guerlac, 97.

ingenious hypothesis of Mr. Newton should not be admitted."[89] Newton had been satisfied that his experiments strongly suggested to him his conclusion—even if it did not technically work to perfection. Those already inclined to oppose him were not persuaded. As Guerlac has explained, Newton distinguished between the "*factual discovery*" of the crucial experiment and the theory of colour.[90] Despite Newton's use of the term crucial experiment, he recognized that further experiment was required to demonstrate his doctrine and in July of 1672 he wrote to Oldenberg to suggest eight "Quaeries" to be investigated by further experiments.[91] His opponents, however, believed that the *experimentum crucis,* as Newton described it, "alone would suffice to prove or disprove Newton's doctrine."[92]

To say that Newton did not care whether or not his facts were accepted would seem to be untenable given the vigor with which he defended the "New theory." When he set about introducing his theory to the public for the first time in 1672, he consciously sought to structure his report in a fashion that would heighten its authority. According to Peter Dear, in the "New theory" "Newton had taken pains to cast the piece in the correct mold, so that it should carry the proper weight," even though the experience he recorded was a "fabrication."[93] Newton had invented such a historical narrative for his experiment because that was the convention at the time for reporting matters of fact.[94] By writing the "New theory" the way he did, Newton hoped to strengthen the credibility of his theory by

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[89] Guerlac, 98-9.
[90] Guerlac, 98.
[91] Newton to Oldenberg, 6 July, 1672, in *Correspondence, vol. I,* 209-10.
[92] Guerlac, 98.
[93] Dear, "*Toitus in Verba*," 154.
[94] Dear, "*Toitus in verba*," 154.

presenting it to his audience in a familiar form. When he failed to achieve consensus, he adapted his approach. He provided more details of the *experimentum crucis* and better explained his theory.[95] When this too failed, Newton bitterly removed himself from the philosophic discourse and cloistered himself at Cambridge. Newton had come out of a mathematical tradition which "privileged the right of a mathematician to keep his work private and to develop the fruits of his labours in private."[96] He exploited this as an excuse to save himself from further debate. As well, by this point he was more interested in studying alchemy and scriptural hermeneutics, which he was disinclined to take public for obvious reasons.[97]

When Newton was finally brought out of isolation to publish the *Principia* in 1687, he did so with an entirely new literary technology that was more closely aligned with mathematics while still maintaining a strong focus on physical experiment. As Dear has suggested with his notion of physico-mathematical philosophy, Newton's method was as much or more a product of as it was part of Baconian tradition. Rather than having been a failed attempt by Newton to meet the conventions of his time, in the "New theory" Newton was in fact trying to subvert the conventions in order to promote his own model of mathematical natural philosophy. Indeed, according to Alan Shapiro, "Newton was as concerned with reforming the methods of natural science as with the science itself."[98] Though he might have failed with the "New theory," it was merely the first attempt by

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[95] Iliffe, "Is He Like Other Men?" 162.
[96] Iliffe, "'In the Warehouse': Privacy, Property and Priority in the Early Royal Society," *History of Science* 30 (1992): 33.
[97] Iliffe, "Is he like other men?" 163.
[98] Alan Shapiro, "Experiment and Mathematics," 36.

Newton. He learned from the experience and was much more successful when he published his theory of gravitation.

In the third chapter I argue that the primary issue at hand during the optical controversies was the problem of reception. Thus far I have retained a relatively top-down discussion of knowledge transmission; however, rather than communicated exclusively down from the elite, knowledge is more usefully conceptualized as being circulated. The scientific audience plays a critical role in the process by which an experimental report becomes a matter of 'fact' because it is the audience that gives scientific facts 'meaning.' As such, I begin to take up Jim Secord's challenge to historians of science to explore how knowledge was transmitted back to the 'centre' from the periphery and address the question of reception and the role of the scientific audience.[99]

[99] Jim Secord, "Knowledge in Transit," *Isis* 95 (2004): 654-672.

Chapter Three:
A Seat More Convenient for Persons of Quality: The Problem of Reception and the Location of Authority

> "Nature and Nature's laws lay hid in night;
> God said, "Let Newton be!" and all was light"
> -Alexander Pope, *Epitaph Intended for Sir Isaac Newton*

When Isaac Newton became president of the Royal Society in 1703, the Society had suffered from years of neglect and administrative disorganization. Amongst his most pressing concerns was the Society's imminent eviction from Gresham College so that it could undergo renovations. In an effort to secure new, suitable quarters for the Society, Newton composed a letter to Queen Anne requesting funds. He contended that a space was necessary for the Royal Society to be able to carry out its mission arguing that "a seat nearer Westminster would be more convenient for persons of Quality & render ye meetings more numerous & thereby conduce more of the improvement of natural knowledge."[1] Newton clearly considered natural philosophy to be an elite enterprise in which the proper audience was socially exclusive.[2] However, Newton had expressed a rather different view in a 1676 letter to Robert Hooke. Instead, Newton eschewed the 'public' experiments in the Royal Society. He argued that it was preferable for natural philosophy to be conducted via "private correspondence." After all, he stated, "what's done before many witnesses is seldome wthout some further concern then that for truth:

[1] Royal Society, Dom. MSS V, f. 45, Newton to Queen Anne, a draft, n.d. For background on Newton's petition see A. Rupert Hall, *Isaac Newton: Adventurer in Thought* (Oxford: Blackwell Press, 1992), 306-7.
[2] Larry Stewart, "Other Centres of Calculation, or, Where the Royal Society Didn't Count: Commerce, Coffee-Houses and Natural Philosophy in Early Modern London," *The British Journal for the History of Science* 32 (1999): 147.

but what passes between friends in private usually deserves ye name of consultation rather than contest."[3]

By the eighteenth century, when the letter to Queen Anne was written, Newton had secured his own audience, which had been part of his purpose when writing the "New theory." Thus, the 1670s optical disputes should be seen as a problem of reception and as part of Newton's difficult process of establishing control over his audience. Fundamental to the dispute was Newton's pronouncement of the solution to a 'fatal' flaw in previous optical theories. According to Newton the shape of the spectrum that the crucial experiment demonstrated was fundamental problem that a successful theory of light and colour had to account for; however, no one before Newton had even noticed this supposedly critical problem. Newton failed to recognize that he had come to his theory gradually and not as an immediate revelation as the "New theory" suggested. Since Newton was relatively unknown, the onus was on him to prove the validity of his work. As Larry Stewart has observed:

> The refraction of Newton's sunbeams revealed that written reports alone could not guarantee assent. It was not necessarily the experiment that was crucial of itself but the audience at the event or at its replication.[4]

The audience one commanded played a crucial role in one's ability to obtain credibility, which was critical for an idea to be received as desired. Newton accepted the social exclusivity for natural philosophy held by Robert Boyle. Indeed, he went further than Boyle who was a constant advocate for a natural philosophy that was open and

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[3] Isaac Newton to Robert Hooke, 5 February, 1675/6, *The Correspondence of Isaac Newton, vol. I,1661-1675*, edited by Henry Turnbull (Cambridge: Cambridge University Press, 1959), 416.
[4] Larry Stewart, *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (*Cambridge: Cambridge University Press, 1992)*, 104.

accessible.[5] Perhaps due to his mathematical background, Newton was more concerned with the 'truth' of matters of fact than that a new science was open to the public.[6] Furthermore, Newton sought to separate personal status from experimental facts. In this chapter I use the question of reception in order to investigate the role of the audience and the location of authority in the making of knowledge in Newton's natural philosophy. I argue that the crucial aspect of Newton's philosophical method was an experimental philosophy that saw experiment as 'objective' and entirely separated from the status and authority of the natural philosopher.

A critical aspect of science that is only just beginning to be truly recognized is the scientific audience. Not only does science have an audience, but, because science does not end "where other forms of culture" begin, the scientific audience is not passive.[7] It plays a role that is not only active but essential to the making of scientific knowledge.[8] As Stewart has observed, "the audience has been undervalued, but audience did not merely absorb."[9] Elsewhere, he has noted that "audiences were the mirror in which

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[5] Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (*Chicago: University of Chicago Press, 1994)*, 176. See also Shapin and Schaffer on Hobbes' rejection that the Royal Society occupied a public space, Shapin & Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985), 113-4.

[6] Rob Iliffe, "'In the Warehouse': Privacy, Property and Priority in the Early Royal Society," *History of Science* 30 (1992), 33.

[7] Steven Shapin, "Science and the Public," in *Companion to the History of Modern Science,* edited by R.C. Olby et al (London: Routledge, 1990), 990. Lissa Roberts has made a similar argument regarding scientific instruments, see Roberts, "A Word and the World: The Significance of Naming the Calorimeter," *Isis* 82 (1991): 198-222.

[8] Steven Shapin, "Audience for Science in Eighteenth Century Edinburgh," *History of Science* 12 (1974): 95-121.

[9] Stewart, "Feedback Loop: A Review Essay on the Public Sphere, Pop Culture, and the Early-Modern Sciences ," *Canadian Journal of History* 42 (2007), 474.

replication was confirmed."[10] Constitution of experimental knowledge was to be a public process.[11] The role of the audience was more than just that of confirming matters of fact. Stewart has proposed the term "feedback loop" to describe how "the public became essential to a process of circulation, a feedback loop that lasted as long as it had something to offer and questions to ask."[12] When studies regarding science and public culture are taken into account, it becomes clear that the role of the audience cannot be restricted to relationships of patronage or elite dissemination.[13] I wish to conceive of the audience broadly in order to consider the audience for seventeenth-century scientific texts, which, as Simon Schaffer has noted, are used by scientists "as knowledge producing tools."[14] Texts and audience are fundamentally interrelated in the operation of science; therefore, it is essential that a study of literary technology also recognize the audience.

Thomas Kuhn's *Structure of Scientific Revolutions* marked a crucial moment for the historiography of science. It paved the way for the history of science to be wrested away from the domain of scientists to that of social sciences and humanities.[15] According to Kuhn, the traditional view of science was that it is cumulative and each successive

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[10] Stewart, "The Laboratory, the Workshop, and the Theatre of Experiment," in *Science and Spectacle in the European Enlightenment*, edited by Bernadette Bensaude-Vincent and Christine Blondel (Aldershot, UK: Ashgate, 2008), 14.
[11] Shapin and Schaffer, 39.
[12] Stewart, "Feedback Loop," 466.
[13] See Michael Lynn, *Popular Science and Public Opinion in Eighteenth-Century France* (Manchester: Manchester University Press, 2006); Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820* (Cambridge: Cambridge University Press, 1992); Larry Stewart, *Rise of Public Science.*
[14] Simon Schaffer, "The Leviathan of Parsontown: Literary Technology and Scientific Representation," in *Inscribing Science: Scientific Texts and the Materiality of Communication,* edited by Timothy Lenoir (Stanford: Stanford University Press, 1998), 182.
[15] Thomas Kuhn, *The Structure of Scientific Revolutions, 2nd ed.* (Chicago: University of Chicago Press, 1970).

discovery and theory is built on previous work in a clear procession to the present. Scientists have tended to assume "scientific knowledge is the crowning achievement of human reason."[16] Following from Kuhn has been an argument, by those historians of science Jan Golinski has termed "constructivists," that scientific knowledge is not 'discovered,' absolutely objective knowledge. Instead, it is constructed based on a specific set of exterior elements such as social relationships, geography and politics.[17]

Bruno Latour, in particular, has recognized the central role that scientific publications play in "fabricating" or, to use a less pejorative word, "constructing" facts.[18] He has argued that in order for a paper to be turned into fact it must be both read and responded to.[19] An article without engagement cannot be fact; science is inherently communicative.[20] It cannot be fact without communication because it is the interaction that gives it meaning. Latour has, thus, usefully noted the importance of the "*flow* of objects and concepts through the *network* of participating allies and social worlds."[21] His approach, however, has focused on the "translation of the concerns of the non-scientist into those of the scientist" in a way that is ultimately "kind of funneling" to use Susan

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[16] Ian Hacking, "The Rationality of Science After Kuhn," in *Scientific Inquiry: Readings in the Philosophy of Science*, edited by Robert Klee (Oxford: Oxford University Press, 1999), 216.
[17] Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science, 2nd ed.* (Chicago: University of Chicago Press, 2005). On objectivity see Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007). On differing modes of objectivity see Alan Megill, *Historical Knowledge, Historical Error: A Contemporary Guide to Practice* (Chicago: University of Chicago Press, 2007), 112-24.
[18] Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, MA: Harvard University Press, 1987), Ch. 1.
[19] Latour, *Science in Action*, 40.
[20] James Secord, "Knowledge in Transit," *Isis* 95 (2004), 664.
[21] Susan Leigh Star and James Greisemer, "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39," *Social Studies of Science* 19 (1989): 389.

Leigh Star and James Greisemer's description of Latour's actor-network model.[22] For Latour the role of the non-scientist is limited to providing concerns for the scientist to 'translate.' Similarly, Simon Schaffer has regarded the audience something that was to be manipulated and controlled by natural philosophers.[23] Such a model fails to recognize fully the part played by the audience. Science cannot operate without the audience because it is the audience that gives it authority and credibility. As Andrea Rusnock has noted, "publishing a paper meant that the 'public,' whoever that might be, would be responsible for evaluating its content."[24] She has expanded on Michael Hunter's argument that the Royal Society "developed as a legitimating body…for scientific reports" in order to propose that correspondence was the Society's primary role.[25] Letters allowed for a more flexible and participatory science than did the *Philosophical Transactions*. The audience did more than legitimate specific matters of fact; it ultimately was needed to legitimize the scientific method itself. Furthermore, I argue that the process of legitimization served to shape natural philosophy.[26]

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[22] Star and Greisemer, 389-90.
[23] Simon Schaffer, "Natural Philosophy and Public Spectacle in the Eighteenth Century," *History of Science* 21 (1983): 1-51.
[24] Andrea Rusnock, "Correspondence Networks and the Royal Society, 1700-1750," *The British Journal for the History of Science* 32 (1999), 163.
[25] Rusnock, 156; Michael Hunter, "Promoting the New Science: Henry Oldenberg and the Early Royal Society," in *Establishing the New Science: The Experience of the Early Royal Society* (Woodbridge, UK: The Boydell Press, 1989).
[26] One of the clearest examples of the audience shaping what experiments were carried out can be found with Robert Hooke whose experiments in *Micrographia* were consciously chosen in accordance with the interests of the Fellows of the Royal Society, see John Harwood, "Rhetoric and Graphics in *Micrographia*" in *Robert Hooke: New Studies,* edited by Michael Hunter and Simon Schaffer (Oxford: The Boydell Press, 1989), 128-30.

The early Royal Society of Robert Boyle had "no clear way forward to making universal knowledge about the structure of nature."[27] Newton sought to solve this problem. Hunter has argued that the understood role of the Royal Society was "to validate the work of others."[28] One of the major social roles it performed was to enforce the experimental method as the means of establishing matters of fact. It must be remembered, however, that the experimental method was by no means secured as the best way of learning about the world in the seventeenth century.[29] Indeed, one of the reasons why Newton's optical controversies have been of such interest to historians of science is the way in which the disputes crucially were about how experimentation itself ought to be carried out. The optical controversies "showed how hard it was for Newton to achieve authority over his 'publick.'"[30]

Newton's optical disputes were centrally an issue of reception. As Latour has contended, for a proof to be proven decisive "there must be *more than one actor*."[31] Reception is therefore of crucial importance to the scientific project. This is most clearly seen with the crucial experiment. The Lèige experimentalists and Robert Hooke perceived the "New theory" in fundamentally different ways. Robert Hooke rejected that the crucial experiment was, in fact, a crucial experiment; therefore, much of the dispute between him and Newton focused on Newton's attempt to demonstrate that it was one.

[27] Peter Dear, "Narratives, Anecdotes and Experiments," in *Literary Structure of Scientific Arguments: Historical Studies*, edited by Peter Dear (Philadelphia: University of Pennsylvania Press, 1991), 162.
[28] Michael Hunter, *Establishing the New Science*, 212.
[29] Shapin and Schaffer.
[30] Schaffer, "Glass Works: Newton's Prisms and the Uses of Experiment," in *The Uses of Experiment: Studies in the Natural Sciences*, edited by David Gooding, Trevor Pinch and Simon Schaffer (Cambridge: Cambridge University Press, 1989), 87.
[31] Bruno Latour, *The Pasteurization of France*, translated by Alan Sheridan and John Law (Cambridge, MA: Harvard University Press, 1988), 15.

The Lèige Jesuits never questioned that the *experimentum crucis* was actually such an experiment. Because of this, they concentrated their efforts on challenging Newton's matters of fact. The distinction is important. Since Hooke rejected the cruciality of the experiment, he was able to accept it as a true report of an experiment while denying Newton's theory. Newton's Liège opponents, by contrast, sought to disprove Newton's theory by showing that his crucial experiment itself was wrong.

In the Liège dispute, Lucas and Linus attempted to follow protocol and were quick to point out that their experiments had been performed before many witnesses. As John Gascoines wrote to Newton:

> For [Linus] hath try'd it again and again, and he *called divers on purpose to see it*, nor ever made difficulty to shew to any one, who either by chaunce came to his chamber as he was doing it, or shewed the least desire to see that same…we think it probable he hath tried his experiment thrice for Mr Newton's once.[32]

Newton, however, was not impressed. While Linus may have performed his experiments before witnesses, the credibility of these observers was questionable from an English perspective as they were generally Jesuits and/or Cartesians. Furthermore, Newton criticized Linus for relying on "old Expts" and long-dead witnesses.[33] Newton may not have held witnessing with quite the same regard as did Boyle, but he still recognized that it played a significant role in the verification of experimental facts. Ultimately, "ye business being about matter of fact was not proper to be decided by writing but by trying

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[32] John Gascoines to Henry Oldenburg, 15 December, 1675, in *Correspondence, vol. I,* 394. Emphasis added.
[33] Newton to Oldenburg, 10 January, 1675/6, in *Correspondence, vol. I,* 410. See also Iliffe, *Newton: A Very Short Introduction* (Oxford: Oxford University Press, 2007), 53.

it before competent witnesses."[34] This preface of competent was critical as the witnesses provided by Linus were not as far as Newton was concerned. It was not enough that an experiment was witnessed; who the witnesses were also mattered.

Boyle, Steven Shapin has claimed, embodied the 'new science' more than any other early experimentalist. Moreover, he "did not *take* on the identity of experimental philosopher, he was a major force in *making* that identity."[35] However, Isaac Newton quickly supplanted Boyle as the embodiment of the experimental natural philosopher and Newton's authority defined much of the eighteenth century. This thesis has sought to chart the transition from Boylean to Newtonian natural philosophy. Crucial to this shift was the role accorded to social status. For Boyle, credibility and gentility were all but interchangeable. To be a natural philosopher was to be a gentleman. Indeed, Shapin demonstrates this succinctly by pointing out that Robert Hooke was not a gentleman and, as such, was not considered by his contemporaries to have been a natural philosopher. Even Hooke seldom referred to himself as such.[36]

Much scholarship has been devoted to trying to define just what made one a gentlemen.[37] In the English Christian conception of gentility, a "gentleman is a Man of himself," on "that is God's *Servant*, *the World's Master,* and his *own man.*"[38] His "own man" was key. Ultimately, the status of a gentleman is most crudely reduced to wealth

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[34] Newton to Oldenburg, 10 January, 1675/6, in *Correspondence, vol. I,* 410.
[35] Shapin, *Social History of Truth,* 126-7.
[36] Shapin, "Who Was Robert Hooke?" in *Robert Hooke: New Studies*, edited by Michael Hunter and Simon Schaffer (Woodbridge, U.K.: The Boydell Press, 1989), 253. See also Rob Iliffe, "Material Doubts: Hooke, Artisan Culture and the Exchange of Information in 1670s London," *The British Journal for the History of Science* 28 (1995): 285-318.
[37] See Shapin, *Social History of Truth,* Ch. 2; Philip Carter, *Men and the Emergence of Polite Society, Britain 1660-1800* (Harlow, UK: Longman, 2001), Ch. 1. Esp. pp. 32-52.
[38] Shapin, *Social History of Truth,* 49.

and blood, or, more cynically, that one is a gentleman because other gentlemen say one is. Yet, wealth did not make one a gentleman so much as it afforded one the freedom to be one. To be a gentleman was not to be beholden to anyone, which required enough wealth to be independent. A gentleman was one for whom others worked who "could be recognized for his idleness."[39] Robert Boyle carried out his experiments because of an innate curiosity and not for any income, while Robert Hooke and Isaac Newton maintained a professional employment that precluded genteel status.

The relevant question, however, is not what made one a gentleman, but what did it mean to be a gentleman? Why did being one matter? That it did matter is evident by the great deal of attention given to it. Gentility, Shapin suggests, mattered because it was perceived as a "Golden Mean," as an 'ideal' of morality in society.[40] This was not to say that gentlemen were necessarily more virtuous than the rest of society, just that their position in society offered an opportunity for a virtuous life not available to others of lesser social status. By the mid-seventeenth century, the notion of the gentleman reflected a society in flux. Philip Carter has suggested an idea of 'gentlemen' that evolved to mirror the growing social mobility of the seventeenth and eighteenth centuries. According to Carter, to be a gentleman was an ideal of politeness and refinedness. It was no longer tied to matters of birth; instead, there was a:

> "redeployment of titles—'Mr' and 'Mrs'/'gentleman' and 'lady'—from individuals who traditionally occupied specific positions and performed specific social duties to anyone, regardless of profession or social background, laying 'claim to a degree of rank and respectability.'[41]

[39] Shapin, *Social History of Truth*, 51.
[40] Shapin, *Social History of Truth*, 51.
[41] Carter, 6.

In a world where one was able to better one's station in life, "concept of gentle manliness [was] based as much on personality as on birth."[42] The rather traditional notion of gentlemanliness that Boyle emphasized was, in many ways, becoming a thing of the past by the time that Boyle rose to prominence.

According to Barbara Shapiro, John Wilkins—who was a founding member of the Royal Society—saw science to be a "leisure-time occupation with some practical application" for the socially elite.[43] Usefulness was applauded, but served only a tangential purpose. Similarly, Boyle's conception of the Christian virtuoso often relegated utility to be merely a happy by-product. Gentlemen required morally appropriate activities and natural philosophy offered just such. It might have also allowed for mechanical or practical improvements in daily life, but its purpose, for Boyle, was theological at its core. Steven Shapin has contended that Boyle sought to defend natural philosophy by following "the general drift of late Tudor and Stuart Christian moralism in arguing for the necessity of discipline and vocation."[44] Boyle argued that all men required a calling as it was a "souueraigne Preseruatiue agenst Idleness."[45] This suggests that Boyle was more concerned with the production of virtue than he was with what would be regarded as 'scientific' accomplishment today. After all, "avoidance of idleness was both a divine obligation and a way of achieving virtue."[46] If Gentlemen did not have morally

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[42] Carter, 57.
[43] Barbara Shapiro, *John Wilkins, 1614-1672: An Intellectual Biography* (Berkely: University of California Press, 1969), 31.
[44] Shapin, *Social History of Truth*, 102.
[45] Robert Boyle, "Aretology," in *The Early Essays and Ethics of Robert Boyle*, edited by John Harwood (Carbondale, Il: Southern Illinois University Press, 1991), 85.
[46] Shapin, *Social History of Truth*, 103.

appropriate activities they would “make vacation their only vocation” and to be an idle man was to be “the most inexcusable of All men.”[47] Natural philosophy offered an ideal diversion from idleness. Frequent dispute or mistrust would have been inherently inappropriate for such a moral enterprise.

Boyle assumed a certain moral superiority to be possessed by gentlemen. Carter has argued that there was an idea “that changes in personal conduct were determined by one’s socio-economic environment.”[48] Carter’s argument lends credence to Shapin’s proposition that Boyle’s regard for gentility was widespread in seventeenth century Britain. From a standpoint of ‘scientific’ advancement, the Society would have benefitted from attempting to recruit into its numbers members of the trades. Indeed, one of its first projects was an ultimately abortive “History of Trades.”[49] However, the Society instead actively kept itself a highly elite institution.[50] The History failed in part because “artisans were not sufficiently conversant with the arcana of gentlemen philosophers.”[51] Following arguments that have been made by Margaret Jacob and James Jacob, one might derive the position that the leading members of the Society saw it as serving an essentially political purpose rather than a ‘scientific’ one.[52]

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[47] Boyle, “Aretology,” 88; Boyle, “Of Time and Idleness,” in *Early Essays and Ethics,* 242.
[48] Carter, 27.
[49] See Walter Houghton, Jr., “The History of Trades: Its Relation to Seveenteenth-Century Thought,” *Journal of the History of Ideas* 2 (1941): 33-60; Kathleen Ochs, “The Royal Society of London's History of Trades Programme: An Early Episode in Applied Science,” *Notes and Records of the Royal Society* 39 (1985): 129-58.
[50] Hunter, *Establishing the New Science,* 11-2.
[51] Stewart, *Rise of Public Science,* 14. Cf. Robert Boyle, “The Usefulness of Natural Philosophy and sequels to Spring of the Air (1662-3),” in *The Works of Robert Boyle, Electronic Edition, vol. III,* edited by Michael Hunter and Edward B. Davis (Charlottesville, VA: InterLex Corporation, 2003). Rob Iliffe has noted that the suspicion was mutual, see Ilffe, “Material Doubts.”
[52] James Jacob and Margaret Jacob, “Anglican Origins of Modern Science: The Metaphysical Foundations of the Whig Constitution,” *Isis* 71 (1980): 251-67.

The notion of the gentleman was fundamental to Boyle's writing. He routinely made reference to this or that 'ingenious' gentleman and the matters of fact they had reported to him. An interesting early example can be found in the correspondence between Boyle and his early mentor Samuel Hartlib during the 1650s, during which time Hartlib was suffering significantly from the stone and was experimenting with various possible treatments. In his descriptions of different cures Hartlib generally provides Boyle with the name of the gentleman or gentlewoman from whom he had been given the medicine. For instance, he described how "gentlewoman" had "assured me, upon her own knowledge" that a "young gentlewoman" and an "ancient gentleman" had been cured "by the only use of Turkish drink coffee."[53] Boyle continued to maintain an interest in medicine throughout his life. He was frequently consulted by physicians and was forever testing different treatments, many of which were rather more superstitious than medicinal by modern standards. As G.W. Jones saw it, Boyle's "gullibility and credulity" appear as distinct features of his medical writings.[54] This would seem odd considering his esteemed status as a chemist and the regard he held for skepticism and experimental rigour; however, Jones did not consider how Boyle understood credibility. Boyle took medical advice from people whose status indicated a degree of trustworthiness. Furthermore, a suggested medicine could not be judged without being tried; however, many of the more esoteric remedies Boyle described he was not in a position to test personally.

[53] Samuel Hartlib to Robert Boyle, nd. In *The Works of the Honourable Robert Boyle, Vol. VI,* edited Thomas Birch (J & F Rivington, 1772), 95.
[54] G.W. Jones, "Robert Boyle as a Medical Man," *Bulletin for the History of Medicine* 38 (1964), 143.

When it came to experimental philosophy Boyle was less credulous and generally did not transmit matters of fact, at least without reservations, that had been reported to him without confirming the experiments himself if they seemed too spectacular. An example of this can be found in his essay “Of the Intestine Motions Of the Particles of Quiescent Solids.” In this essay he described how “an ingenious Gentleman of my acquaintance” had told him “he had a Turquois-stone, which if he were not mistaken had a wonderful property, for there being in it several spots of Colours differing from the rest of the Gem, these spots seem'd, though very slowly, to move from one part of the stone to the other.”[55] In order “to ascertain my self of the truth of it,” Boyle borrowed the stone to observe the phenomena. Once in possession of the stone, he employed an “ingenious youth” to keep watch on the stone and draw occasional pictures of the spots moving. After several weeks of observation, it was “unanimously concluded” that the spots did indeed move.[56] Such testimony was insufficient for Christiaan Huygens, who wrote to Henry Oldenburg that he “should require some very authentic and carefully verified attestations to it.”[57] Oldenburg’s response was simply to reassert Boyle’s assurances that the observations had been made in “good faith,” having been witnessed by Hooke “amongst others,” and to remind Huygens of Boyle’s status.[58] As Steven Shapin has argued, status also played an important role with whom one was allowed to argue.

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[55] Boyle, “Essay Of the Intestine Motions Of the Particles of Quiescent Solids,” in *Works, vol. VI,* 201.
[56] Boyle, “Quiescent Solids,” 202.
[57] Christiaan Huygens to Henry Oldenburg, 12 January, 1670, in *The Correspondence of Henry Oldenburg, vol. VI,* edited by A. Rupert Hall and Marie Boas Hall (Madison, WI: University of Wisconsin Press, 1969), 426.
[58] Oldenburg to Huygens, 31 January, 1670, in *Correspondence of Henry Oldenburg,* 460. See also Shapin, *Social History of Truth,* 293.

Oldenburg was careful to remind those who might forget the rules of comportment "that publicly expressed distrust might be consequential."[59]

A gentleman such as Robert Boyle was to be considered inherently trustworthy because he did not stand to gain anything personally from the matters of fact he reported. Boyle's social standing ultimately had as much to do with his birth and personal wealth as it did his ability as a natural philosopher. His participation in experimental philosophy was a matter of personal and genuine curiosity. The inference that Boyle expected his audience to make was that he was trustworthy because he had no reason to lie—unlike someone of lesser status who may have been seeking to advance their career or to benefit financially. In fact, Boyle made a similar argument as to why those of sufficiently low social status might also be trustworthy sources of information. Such individuals possessed neither the means to profit from the facts they reported nor the philosophic knowledge necessary for theoretical speculation. Thus, according to Boyle at least, they were limited to reporting what they actually saw. For example, Boyle regarded reports from pearl divers regarding the effects of water pressure as credible.[60] Servants, on the other hand, held a rather more complicated position. They could be "distrusted costlessly," thereby performing the useful task of protecting the integrity of the gentleman experimentalist.[61] In the case of experimental failure, Boyle often sought to demonstrate that it was his assistant who was to blame, which was often the only time that Boyle acknowledged them at all in his writing.[62] As Shapin has shown, to reject a

[59] Shapin, *Social History of Truth,* 307.
[60] Shapin, *Social History of Truth,* 260-6.
[61] Shapin, *Social History of Truth*, 392.
[62] Shapin, *Social History of Truth,* Ch. 8.

gentleman of science's reported fact was to question his authority and, by extension to call into question the entire basis of natural philosophy.[63]

Ironically, Robert Hooke would seem to have been just the sort of person that, according to Boyle's logic, would have been the least trustworthy. Hooke was highly knowledgeable, and thus able to develop complex theories which depended on the truth of his reported matters of fact. Even more damning, however, Hooke derived his status almost entirely from his role as an experimental expert.[64] Furthermore, he was intimately engaged with the trades, which Boyle suggested to be an unreliable source of knowledge, and Hooke aggressively sought material gain from his discoveries.[65] Hooke did not have the advantage of status. In order to compensate for this 'deficiency,' he emphasized his position as an expert. When he wrote that "for all the expts & obss: I have hitherto made" and he that "did not take up" his theory "without first trying some hundreds of expts" Hooke was reminding his audience that he had long experience as an experimental philosopher.[66] He was the one with the established credentials while Newton was the lowly upstart who needed to prove himself. As both assistant to Boyle and curator of experiments for the Royal Society, Hooke had had the opportunity to demonstrate his prowess before many of Europe's leading natural philosophers. His social status, however, was tenuous and his position was defined according to his relationship with those upon whom he was dependent.[67] The ambiguity of his station left him frequently

[63] Shapin, *Social History of Truth*, 266-91, esp. 287-91.
[64] Shapin, "Who Was Robert Hooke?" 253-4.
[65] See Iliffe, "In the Warehouse" for an example of Hooke aggressively pursuing the commercial interests of his experimental discoveries.
[66] Hooke to Oldenburg, 15 February 1671/2, *Correspondence, vol. I,* 110.
[67] Shapin, "Who Was Robert Hooke?" 253.

feeling abused by his socially superior fellows at the Royal Society who often saw fit to call his work into question when it suited them or to make demands on him that he felt beneath his expertise.[68] Despite the fact that Hooke was "perhaps the only essential member of the Society," he was consistently treated as a "paid employee."[69] At the same time, however, his standing as a skilled experimentalist was never in question.

While Hooke was an established and well-known member of the scientific community, Isaac Newton was an obscure mathematics professor with few public credentials. With this in mind Hooke's infamous intimation that "Mr. Newton had taken" Hooke's "hypothesis of the puls or wave" can be considered in a rather different light.[70] Rather than concentrating on the accusation of plagiarism or on Hooke's disingenuous representation of Newton's theory (which was not a pulse or wave theory at all), I wish to point to the way in which Hooke was seeking to emphasize that he had been there first; Newton was the latecomer. Newton's work, Hooke suggested, was derived from experiments that Hooke had published years before. Hooke pronounced himself "well pleased to see those notions promoted and improved which I had long since began, but had not time to compleat." Newton had "gone farther" to "compleat, rectify and reform… what were the sentiments of" Hooke's "younger studies."[71] Newton, Hooke intimated, was simply providing an extension to what Hooke had already accomplished. Certainly Newton owed a debt to Hooke's *Micrographia*; however, Newton's "New theory" was entirely original. While he was inspired by some of the observations that Hooke had

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[68] Shapin, "Who Was Robert Hooke?" 257.
[69] Iliffe, "Material Doubts," 289.
[70] Robert Hooke, *The Diary of Robert Hooke, M.A., M.D., F.R.S., 1672-1680*, edited by Henry Robinson and Walter Adams (London: Taylor and Francis, 1935), 206.
[71] Hooke to Newton, 20 January, 1675/6, *Correspondence, vol. I,* 412.

described, Newton used very different instruments and techniques and came to conclusions that were highly dissimilar to those of Hooke.

Boyle located scientific authority in the social status of the person reporting matters of fact. Hooke continued to emphasize the status of the practitioner; however, he focused on his publicly acknowledged expertise to defend his theory. Newton, on the other hand, sought to place the authority of his facts within the facts themselves. His were not credible because of his status, but because they were 'objective' statements about reality. His theory was "not an Hypotheses but most rigid consequence…evinced by ye mediation of experiments concluding directly & without any suspicion of doubt."[72] Newton's conception of authority anticipated the scientific objectivity Lorraine Daston and Peter Galison described as being a product of the nineteenth century. They defined objectivity as "to aspire to knowledge that bears no trace of the knower—knowledge unmarked by prejudice or skill, fantasy or judgment, wishing or striving."[73] Daston and Galison sought to make a strict, epistemological argument which considered using 'objectivity' as a historical category before the 1800s to be anachronistic.[74] Both Lissa Roberts and Jan Golinski, however, have demonstrated that similar notions of objectivity were present in eighteenth century chemistry texts.[75] The process by which science was

[72] Newton to Oldenburg, 6 February, 1671/2, *Correspondence, vol. I*, 96-7.
[73] Daston and Galison, 17.
[74] Daston and Galison, 28-9.
[75] Lissa Roberts, "The Death of the Sensuous Chemist: The 'New' Chemistry and the Transformation of Sensuous Technology," *Studies in History and Philosophy of Science* 26 (1995): 503-29; Golinski, "'The Nicety of Experiment': Precision of Measurement and the Precision of Reasoning in Late Eighteenth-Century Chemistry," in *The Values of Precision,* edited by M. Norton Wise (Princeton: Princeton University Press, 1995). See also Roberts, "Setting the Table: The Disciplinary Development of Eighteenth-Century Chemistry as Read Through the Changing Structure of Its Tables," in *Literary Structure*, 119.

rendered 'objective' began with Newton and his physico-mathematical philosophy even if it was not technically termed as such at the time.

As Shapin has argued, "Boyle's repeated insistence that he had no theoretical investments and prepossessions" was part of his strategy to warrant that "he testified to truth alone."[76] Similarly, Boyle restrained himself from developing theoretical systems. He reported facts that were "the concrete and particular" rather than being caught up in the "abstract and the general."[77] Such lack of theoretical apparatus perhaps allowed him to get away with less stringent consideration for certainty. For Boyle what was important was that he reported experiments accurately. When Boyle or Hooke performed an experiment "they wished to see how nature would behave under previously unobserved, often previously nonexistent, circumstances" and not "demonstrate what was already known."[78] Newton, on the other hand, "selected and utilized [experiments] to elaborate theory."[79] He was not satisfied with simply providing experimental history; therefore, he demanded that his audience cede to his authority much more so than did Boyle. Boyle only asked that they accept that he reported accurately, while Newton expected them to agree with the theory he argued that his matters of fact demonstrated.

When Newton criticised Anthony Linus for citing incompetent witnesses, it might seem to have been at odds with his rather derisive statement to Hooke regarding public

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[76] Shapin, *Social History of Truth*, 235.
[77] Shapin, *Social History of Truth*, 347.
[78] Kuhn, *The Essential Tension*, 43.
[79] Kuhn, *The Essential Tension*, 50.

witnesses.[80] Newton's seemingly contradictory arguments could be read as simply taking whichever approach he deemed most convenient to attack his opponents; however, I wish to argue that Newton did, in fact, maintain a consistent position. Replication was the central issue for natural philosophy and perhaps for no one was this clearer than with Newton. Indeed, the question of replication is what makes the Liège dispute significant. Robert Boyle was able to accept that experiments could only be demonstrated to be probably true, something that Newton's mathematical mind was unable to do. Experiments could not simply be the accounts of discrete events that Boyle described; instead, Newton sought to present his experiments more along the lines of a mathematical proof. The event was irrelevant. What mattered was an experiment was a repeatable incident that occurred *outside* of history. It was this view of experimental philosophy that caused Newton to put so much emphasis on the crucial experiment in the first place and led him to defend it at such length.

Newton did not resolve the conflict created by the *experimentum crucis*; instead, that job fell to Hooke's true heir and Newton's protégée, J.T. Desaguliers. The primary means by which he would do so was by adapting the experiment so that the problems pointed to by dissenters were 'fixed' and then demonstrating the experiment before gentlemen and diplomats in both London and Paris.[81] Newton's original paper was insufficiently detailed for easy replication.[82] As Desaguliers admitted "Some gentlemen abroad" had "complained that they had not found the Experiments answer, for want of

[80] Newton to Oldenburg, 10 January, 1675/6, in *Correspondence, Vol. I,* 410; Newton to Hooke, 5 February, 1675/6, *Correspondence, vol. I,* 416. See also Richard Westfall, "Newton Defends His First Publication: The Newton-Lucas Correspondence" *Isis* 57 (1966): 299-314; Schaffer, "Glass Works," 85-91.
[81] Guerlac, 121-2.
[82] Schaffer, "Glass Works," 79.

sufficient Directions."[83] It was essential that he demonstrate the crucial experiment without resorting to inside knowledge provided by Newton.[84] While the "New theory" lacked the requisite details, Desaguliers was able to rely only on Newton's publicly available accounts due to the additional resource of the *Opticks,* which had provided a method for separating monochromatic rays that had not been published "before Sir Is. Newton's *Opticks* came abroad" in 1704.[85] The experiment was much "less troublesome if it be made in such a manner as is described in the fourth Proposition of the first Book of Sir Is. Newton's *Opticks.*"[86] Such an approach also had the benefit of crediting the competency of the continental experimenters who had been unable to replicate Newton's experiment. It was not because of "their wilful incompetence," as Newton had insinuated, but because they simply lacked the necessary information.[87] Desaguliers' claim that he "had no other Directions" than what he found in the "New theory" and the *Opticks* was a fiction since he possessed direct access to Newton; however, it was effective in its purpose of ending the continental challenge to Newton's credibility.[88]

Desaguliers' paper, published in the *Philosophical Transactions* in 1714, also showed significant differences from Newton's approach. Desaguliers reported the experiments more descriptively and was more careful with his details than Newton had been. While he claimed that could "have referr'd the Reader altogether" to the *Opticks,*

[83] J.T. Desaguliers, "An Account of Some Experiments of Light and Colours, Formerly Made by Sir Isaac Newton, and Mention'd in His Opticks, Lately Repeated before the Royal Society b J.T. Desaguliers, F.R.S." *Philosophical Transactions* 29 (1714-1716): 447.
[84] Schaffer, "Glass Works," 95.
[85] Desaguliers, "Account of Some Experiments," 433.
[86] Desaguliers, "Account of Some Experiments," 435.
[87] Schaffer, "Glass Works," 88.
[88] Desaguliers, "Account of Some Experiments," 447.

Desaguliers' account was successful in part because he finally provided enough information to replicate easily the crucial experiment.[89] Unlike Newton, Desaguliers provided a large number of illustrations and took great care to describe the apparatus used. The improved description and provision of illustrations are examples of Desaguliers harnessing a more effective literary technology than had Newton. Even more important, however, was Desaguliers' demonstration of the crucial experiment before witnesses. The authority of Desaguliers rested primarily on his brilliant manipulation of social technology. As Simon Schaffer has observed, Desaguliers "tailored his experiments for effective witnessing."[90] According to J.L Heilbron, when a group of French natural philosophers came to London in 1715, they "arrived prejudiced" against Newton, but "left Desaguliers' neat show fully satisfied with Newton's doctrine."[91]

The true strength of Desaguliers, according to I. Bernard Cohen, was Desaguliers' ability to demonstrate Newtonian philosophy "without mathematics."[92] Desaguliers wrote for those "little versed in mathematical sciences"[93] and, therefore, posited that the truth of Newtonian philosophy "is supported by mathematics, yet its physical discoveries may be communicated without."[94] Following Desaguliers' efforts, the crucial experiment had been shown conclusively and was no longer a means by which detractors of Newton were able to attack him. Furthermore, he reconciled the demonstrability of Newton's physical

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[89] Desaguliers, "Account of Some Experiments," 447.
[90] Schaffer, "Glass Works," 95.
[91] J.L. Heilbron, *Physics at the Royal Society During Newton's Presidency* (Los Angeles: William Andrews Clark Memorial Library, 1983), 92.
[92] I. Bernard Cohen, *Franklin and Newton: An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof* (Philadelphia: The American Philosophical Society, 1956), 244.
[93] Desaguliers, *A Course of Experimental Philosophy, Vol. I. 2nd Edition, Corrected* (London, 1745), viii.
[94] Desaguliers, *Experimental Philosophy*,viii.

experimental philosophy with the mathematical difficulty that had previously made Newton unapproachable. Much like Hooke, Desaguliers showed, rather than told, the matters of fact he wished to prove.[95]

In 1718, Willem 'sGravesande wrote to Newton to thank him for a copy of *Optice*, noting that:

> I have had some success in giving a taste of your philosophy in this University; as I talk to people who have made very little progress in mathematics I have been obliged to have several machines constructed to convey the force of propositions whose demonstrations they had not understood.[96]

Similarly to Desaguliers, 'sGravesande's letter pointed to an "inversion of strength from seemingly incomprehensible rational demonstration to allegedly *forceful* experimental performance."[97] Public demonstration allowed 'sGravesande to give "direct proof" of Newton's philosophy.[98] J.T Desaguliers asserted the authority of public demonstration over mathematics even more boldly. Newton's mathematical demonstrations had served to provide the theoretical underpinnings of Newton's philosophy; however, Desaguliers claimed that demonstration was enough to allow an audience to understand and participate in Newtonian philosophy.

Desaguliers provided a crucial role for the Royal Society by tying "his public performance" to its needs and giving the general audience confidence in the experimental

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[95] On Hooke, see Stewart, *Rise of Public Science*, 13.
[96] Willem Jakob 'sGravesande to Newton, 13/24 June 1718, in A. Rupert Hall, "Further Newton Correspondence," *Notes and Records of the Royal Society of London* 37 (1982): 26.
[97] Simon Schaffer, "Public Experiments," *Making Things Public: Atmospheres of Democracy,* edited by Bruno Latour and Peter Weibel (Cambridge, MA: The MIT Press, 2005), 306.
[98] 'sGravesande to Newton, 13/24 June 1718, "Further Correspondence," 26.

method.[99] Moreover, he completed the transition from Boyle's to Newton's conception of authority. Even more usefully to eighteenth-century natural philosophers, he did so while eliminating Newton's emphasis on mathematics. Desaguliers chose to take seriously the Queries of Newton's *Opticks* as important avenues of experimentation. In doing so he "also succeeded in transforming the meaning of Newtonianism" and, by extension, natural philosophy.[100] By removing the mathematical veil that had obscured Newton's philosophy, Desaguliers suggested that anyone could participate. The careful and complicated mathematical demonstrations that Newton had so much depended upon were no longer necessary or perhaps even relevant. Mathematics had been essential when Newton was establishing his credentials, but by the time Desaguliers rose to prominence Newton's authority in England was all but absolute. The power of experimental facts no longer resided in gentility or even expert status *per se*; instead, facts were to be regarded as 'objective' statements about the world and they were visibly shown as such through public demonstration.

[99] Stewart, *Rise of Public Science*, 121.
[100] Stewart, *Rise of Public Science*, 130.

Conclusion

"To Newton's genius, and immortal fame
Th'adventurous muse with trembling pinion soars"
– Richard Glover[1]

Following his death in 1691, Robert Boyle's will stipulated the establishment of an annual series of lectures intended "for proving the Christian religion against notorious infidels, viz, Atheists, Deists, Pagans, Jews, and Mahometans."[2] When Richard Bentley was to deliver the first of these lectures in 1692 he turned to Isaac Newton for assistance in using Newton's system as evidence in favour of the Christian God. Newton wrote four letters to Bentley in response to his request. As Newton told Bentley, when he had composed the *Principia* he "had an eye upon such Principles as might work wth considering men for the beleife of a Deity."[3] Bentley was only the first of a number of lecturers who sought to use Newtonian philosophy in order to carry out Boyle's purpose. These lectures were fundamental in "consolidating Newtonian ideology."[4] They played an important part in transforming the *Principia* from an incomprehensible work of natural philosophy into a philosophical model with profound implications for how the world would be understood in the eighteenth century.

It was the Boyle Lectures, according to Margaret Jacob, that "created the Newtonian world view."[5] From that point forward, Newton could no longer be the

[1] Quoted in Patricia Fara, *Newton: The Making of Genius* (New York: Columbia University Press, 2002), 64.
[2] Robert Boyle's will (July, 1691) quoted in Margaret Jacob, *Newtonians and the English Revolution, 1689-1720* (Ithaca, NY: Cornell University Press, 1975), 144. For more on the Boyle lectures see Jacob, Chapters 4 and 5.
[3] Isaac Newton to Richard Bentley, 10 December, 1692, in *The Correspondence of Isaac Newton, vol. III, 1688-1694,* edited by Henry Turnbull (Cambridge: Cambridge University Press, 1961), 233.
[4] Fara, 75.
[5] Jacob, 176.

retiring scholar hidden away in Trinity College, Cambridge who saw "not what there is desirable in publick esteeme."[6] Though the conventional portrayal of Newton has been that he shunned fame, by the eighteenth century he had recognized the importance of developing a public image. Indeed, he sat for some twenty portraits and busts, many of which he paid for himself.[7] Perhaps even more telling is the fact that there are no contemporary images of Robert Hooke. All that survived Hooke's death in 1703 was Richard Waller's rather negative description of Hooke's physical appearance.[8] While Newton went on to near deification, Hooke's legacy quickly diminished. Partially due to a concerted effort by Newton, he was largely forgotten until historians rediscovered him in the mid-twentieth century.[9] By the eighteenth century, it is apparent that Newton had learned the value of a carefully crafted public image.

Following the disastrous results of the "New theory about light and colour," Newton had recognized that it was not enough that his matters of fact be right or that his theory be superior. The eighteenth century was in many ways the Newtonian century. With the publication of the *Principia*, Newton had already begun a transformation into becoming *the* final authority on natural philosophy. From the many portraits he commissioned to the praises heaped on him by the likes of the poet Alexander Pope and the French Enlightenment *philosophe* Voltaire, Newton achieved an "almost God-like

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[6] Isaac Newton to John Collins, 18 February 1669/70, in *The Correspondence of Isaac Newton, vol. I, 1661-1675,* edited by Henry Turnbull (Cambridge: Cambridge University Press, 1959), 27.
[7] Fara, 34.
[8] Patri Pugliese, "Hooke, Robert (1635–1703)," in *Oxford Dictionary of National Biography*, eee ed. H. C. G. Matthew and Brian Harrison (Oxford: OUP, 2004); online ed., ed. Lawrence Goldman, May 2006; "Hooke's Possessions at His Death: A Hitherto Unknown Inventory," in *Robert Hooke: New Studies*, edited by Michael Hunter and Simon Schaffer (Woodbridge, UK: The Boydell Press, 1989), 287-8.
[9] Michael Hunter and Simon Schaffer, "Introduction," in *Robert Hooke: New Studies*, 3-4.

reputation" that left him a nearly unimpeachable authority for centuries following his death.[10] After Newton became president of the Royal Society in 1703 he finally published a complete version of *Opticks* in 1704. Thereafter, his optical theory was increasingly regarded as canon. It was not seriously challenged in England until Thomas Young did so at the beginning of the nineteenth century.[11]

The Newtonians of the eighteenth century extended Newton's influence far beyond esoteric natural philosophy. Newton came to transcend the elite world of the Royal Society and permeated public culture. By the eighteenth century an appeal to Newtonian philosophy had become a primary strategy for securing credibility. Newtonian philosophy became iconic. For example, the fashionable doctor George Cheyne began his career by "aggressively" asserting his "iatromathematical," Newtonian credentials as a means of "making himself known."[12] In 1733 Jacob Acworth, the chief surveyor of the Naval Board, proposed warship hull designs based on Newtonian principles. Similarly, mathematical writer and teacher William Emerson "advocated applying Galilean and Newtonian mechanical principles to the problems of navigation and naval architecture" in his "influential work."[13] In 1790s London, Newton was held up as a bastion of orthodoxy and a symbol of political stability during the Millenarian controversies William

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[10] Hunter and Schaffer, 1.

[11] Henry Steffens, *The Development of Newtonian Optics in England* (New York: Science History Publications, 1977), Ch. 3. See also Casper Hakfoort, *Optics in the Age of Euler: Conceptions of the Nature of Light, 1700-1795* (Cambridge: Cambridge University Press, 1995).

[12] Steven Shapin, "Trusting George Cheyne: Scientific Expertise, Common Sense, and Moral Authority in Early Eighteenth-Century Dietetic Medicine," *Bulletin for the History of Medicine* 77 (2003), 271.

[13] Simon Schaffer, "'The Charter'd Thames': Naval Architecture and Experimental Spaces in Georgian Britain," in *The Mindful Hand: Inquiry and Invention From the Late Renaissance to Early Industrialisation*, edited by Lissa Roberts, Simon Schaffer and Peter Dear (Amsterdam: Koninklijke Nederlandse Akademie van Wetenschappen, 2007), 288.

Herschel's famous forty foot reflector telescope had stirred up.[14] Meanwhile those with Millenarian inclinations like Joseph Priestley "worked hard to modify the prevailing interpretations of Isaac Newton in order to make the connections between natural philosophy and Providence utterly transparent."[15] In the early nineteenth century both sides of a Cambridge theological dispute over whether or not to distribute bibles without the Book of Common Prayer attempted to establish their credibility by positioning themselves as Newtonian and their opponents as specifically *not* Newtonian.[16] Ironically, Newton had become transformed into the very kind of blanket authority against which the Royal Society had been established to combat. The optical controversies Newton endured during the 1670s had played a crucial role in shaping his method and began the process of establishing Newtonianism in Britain.

Through the close study of the dispute between Robert Hooke and Isaac Newton, we are able to appreciate the rhetorical methods used by scientists to establish their authority over the knowledge they produce. In this thesis I have analyzed the early development of Newton's literary technology in order to demonstrate the process by which he sought to gain and maintain control of his theories and reputation. In the "New theory about light and colour" Newton challenged the authority of the Royal Society by suggesting and developing alternative conceptions of experimental credibility, mathematical certainty and dissemination. In order to avoid future disagreements, he

[14] Kevin Knox, "Lunatick Visions: Prophecy, Signs and Scientific Knowledge in 1790s London," *History of Science* 37 (1999): 427-58. For e.g. see criticisms of Richard Brothers on pp. 436-7.
[15] Knox, "Lunatick Visions," 441.
[16] Knox, "Dephlogisticating the Bible: Natural Philosophy and Religious Controversy in Late Georgian Cambridge," *History of Science* 34 (1996): 167-200.

further developed his method for presenting his theories that established him as authoritative.

In order for a theory to have any meaning, it needs to be accepted by the scientific community. What this also means is that science has an audience. The scientific audience has tended to be conceptualized in passive terms in which its sole role is receiving knowledge. However, it plays a crucial role because without the assent of the audience, science has no authority. The scientist does not merely transmit matters of fact to the audience, but instead it must be convinced. In this thesis I have argued the audience has played a key part in shaping scientific knowledge. In the case of Isaac Newton this is seen in the lessons he learned from the failure of "New theory about light and colour." In the *Principia*, he presented his theory of universal gravitation in stark mathematical terms that rendered it almost entirely incomprehensible to his audience. This approach solidified his authority because it forced everyone to go through intermediaries if they were to understand it, allowing Newton to control his audience in a way that he had been unable to do with the more accessible "New theory." Thus, the optical controversies played a key role that historians have hitherto failed adequately to recognize in the shaping of the rhetorical methodology of science. With the methodological innovations provided by Newtonian philosophy, the new experimental science was established as *the* means of 'discovering' natural knowledge during the eighteenth century.

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